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NOTES AND COMMENTS

The Fauna of the Deep Sea

One of the most important publications of the past month is Dr Günther's Presidential Address to the Linnean Society of London, just issued in this Society's *Proceedings*. It is a critical review of our present knowledge of the depths of the sea, by one who has made a life-long study of the subject, and added no small contributions to the solution of the problems in question from the biologist's point of view. Dr Günther treats the subject under geographical headings, being of opinion that this arrangement brings out some points of interest more prominently than a general historical statement would do. He is careful, however, to emphasise the fact that he still believes the deep-sea fauna to be one indivisible whole; such types as seem to be characteristic of some particular region of the ocean being "accidentally or ignorantly imported into the deep-sea fauna," or else forms of which the wide range has not yet been ascertained.

Among the many interesting points discussed, there are two expressions of opinion of great importance from one who has so many qualifications to speak authoritatively. The first relates to the question of the permanence of the abyssal ocean depths; the other refers to the presence or absence of life in the middle-depths of the oceanic waters.

Dr Günther is emphatically in favour of the idea that the abysses of the ocean are not permanent, but may well have changed many times in the past. He declares that the opposite view "cannot be accepted by the student of the terrestrial fauna." He remarks: "I cannot help thinking that our knowledge of the nature of the rocks at the bottom of the sea is, at present, to use a mild expression, most imperfect. Is it not possible that continental rocks at the abyssal sea-bottom are so hidden under the deposit which has been in progress of formation for untold ages, as to prevent us from penetrating to them? Possibly the day may come when borings or some similar operation will be successfully carried

out in the abysses, entirely upsetting our present ideas of the geological nature of the sea-bottom. Besides, we have no other means for accounting for the distribution of the terrestrial fauna, more especially in the southern hemisphere, except by assuming that great changes have taken place in the extent and position of continental land, and, moreover, that these changes were still in progress at periods at which our present fauna, or at least part of it, was already in existence."

The question of the existence of a "mid-water fauna" is considered by Dr Günther to be still an open one, to be "decided only by continued tow-net experiments in great depths of the open ocean, with a bottom of clean ooze or mud." Agassiz, as is well known, maintains that there is no such fauna. Dr Günther, however, observes that the experiments of the distinguished American naturalist "prove too much. His tow-nets came up always empty from the intermediate zones. It is very singular that he should not have caught even some of the dead bodies which, like rain, drop constantly from the surface to the bottom. Further, so far as fishes are concerned, there is no reason why certain forms should not permanently inhabit intermediate zones, inasmuch as also pelagic fishes are undoubtedly free swimmers for nearly the whole of their life, without being tied to the proximity of *terra firma*. The ova of many species which live in the mature stage at the bottom of great depths are pelagic, and hatched at or near the surface. The young continue to live for some time under pelagic conditions (*Plagusiae*, *Leptocephali*, *Polyprion*), but as they grow they descend to the deep sea. It is very improbable that this descent is rapid; it must be gradual in order to allow the physiological functions to get used to abyssal conditions; or, in other words, these fish must live for some time in mid-water."

Finally, Dr Günther adds an appropriate plea for more deep-sea work in the Indian Ocean and the Antarctic regions. The ordinary survey of the seas round British India is now nearly completed, but researches on the fauna have only proceeded just far enough to demonstrate their interest and biological importance. "It seems a pity," as Dr Günther remarks, "that while the experience gained on board the 'Investigator' is at least still partly available in the service, no further benefit should accrue from it for science." In reference to the Antarctic Ocean, he points out that our knowledge of its abyssal life rests merely on six trawlings of the 'Challenger.' We trust that ere long the British Government will be induced to help further in this important biological work. To use Dr Günther's words, "the beneficial influence which every purely scientific undertaking exercises upon mankind reaches far beyond its immediate aim." Science "is the mother as well as the daughter of peace."

The International Zoological Congress, 1898

The meeting of the General Committee appointed to arrange for the next meeting of the International Zoological Congress was marked by an unfortunate lack of union. One well-known zoologist described it as the most turbulent meeting he had ever attended. The Congress is to meet in England next August, and the original arrangement was that it should be held in London, which, for various reasons, has now been altered to Cambridge. The success of a similar Congress in America in 1891 was seriously affected by a change in the place of meeting, which led to the abstention of a great number of American men of science, who objected to the alteration. Cambridge does not now occupy the position in the English zoological world which it did in the days of F. M. Balfour. There are no doubt strong reasons for the selection of Cambridge, although the town is not central. But remembering the consequences of the change of locality of the American Congress in 1891, the advocates of Cambridge might have done their best to conciliate provincial representatives. Their attitude was decidedly the reverse. When, for example, Colonel Wardlaw Ramsay proposed that in order to secure one Scottish member on the executive, Sir William Turner should be elected a vice-president, Professor Newton formed the minority of one who voted against it. Professor Poulton, also anxious to make the committee more representative, proposed that the presidents of the Linnean and Entomological Societies should be *ex officio* vice-presidents, which secured at once the warm support of the meeting. The chairman, however, expressed himself confident that Mr Poulton, on thinking the question over, would see the advisability of withdrawing his resolution and allowing the executive committee to select itself the additions to its number. But Mr Poulton remarked that he did not see the advisability of his withdrawing his resolution, and thought it much better not to trust the executive committee to make the additions recommended. The resolution was carried by an overwhelming majority. So the cut and dried plans of those who had arranged the agenda were not accepted quite as they stood. A member who proposed one of the official resolutions read it out as "the resolution I have been 'instructed' to propose;" and then recommended it to the meeting by one hostile criticism. Dr Murie proposed that the executive committee should be composed of an equal number of members from England, Ireland and Scotland, a remarkable suggestion that only fell non-seconded because the zoologist who attempted the feat could not complete his sentence until the meeting was well advanced in the consideration of the next business. A few proposals of this sort, by claim for zoological independence for Wales and the Manxmen, kept the meeting merry. But it seems

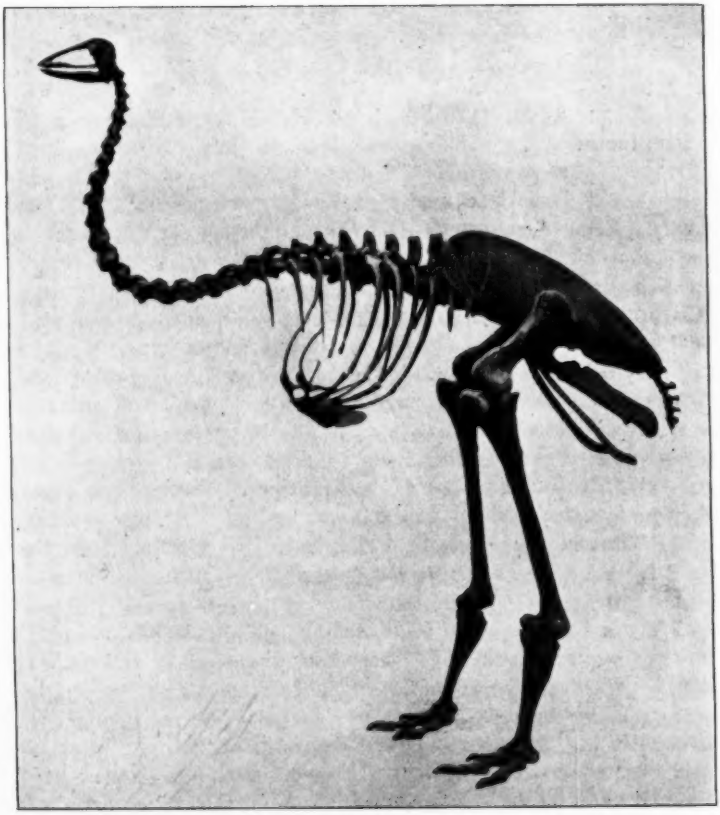
quite clear that the provincial and non-official London zoologists view the present committee with suspicion, as not sufficiently representative of British zoology. Sir John Lubbock has succeeded Sir William Flower in the presidency, and he may be trusted to prevent any further irritation of the majority by a tactless disregard of its manifest wishes. It is a source of the deepest regret to all British zoologists that Sir William Flower himself should be compelled, by need of rest, to refrain from any active part in the arrangements.

Stratigraphical Geology

In connection with Sir Henry Howorth's articles on Geological Nomenclature which we are now publishing, attention may be directed to a paper by Dr Charles R. Keyes, recently read before the St Louis Academy of Sciences, and abstracted in *Science* for October 29 (N.S., vol. vi., p. 655). Dr Keyes declares that "for more than a score of years that branch of geology called stratigraphy has been practically at a standstill. Its methods are the same that were used fifty to seventy-five years ago." At last, however, the problems of the correlation of sedimentary rocks can be attacked in a new way suggested by the field-work of many American geologists. Organic remains, it appears, may now be entirely omitted from consideration, and the relative age of the various strata can be determined solely by observing the succession of geographical changes in the various large areas under comparison. These new methods, Dr Keyes remarks, are more or less complex and far from simple; but he is hopeful that they will eventually lead to a really natural classification of the rocks and definitely put an end to what has been aptly termed 'parochial geology.' He is especially sanguine as to the value of the results to be obtained from a detailed study of the phenomena of mountain-formation. We cannot follow the whole argument from the brief abstract; but any advance in methods which will enable us to restore the geographical features of wide areas of the earth's surface at different successive geological periods will not only make a new era in geological science but also contribute most materially towards the solution of some of the perplexing problems of zoology.

The Geology of Patagonia

This leads us to refer again to the question of the Tertiary deposits of Patagonia and their remarkable mammalian fossils, discussed by Dr Florentino Ameghino in our October number (p. 256). Mr J. B. Hatcher, who has spent much time in studying this southern extremity of the American continent, now expresses the opinion that



RESTORED SKELETON OF AN EXTINCT STRUTHIOUS BIRD
Aepyornis hildebrandti
From a Peat-deposit, Sirabé, Central Madagascar

[*Obtained for the British Museum (Natural History) by* Dr FORSYTH MAJOR]

there is no geological basis for Ameghino's asserted discovery of the Cretaceous ancestors of the mammalia in that region (*Amer. Journ. Sci.* [4], vol. iv., pp. 327-354, Nov. 1897). He finds, apparently, Jurassic rocks there on the Mayer river; he also identifies the supposed Cretaceous series containing Dinosaurian bones. When in Patagonia, however, he never discovered either a mammal bone or a tooth in the deposits yielding Dinosaurian remains, and he arrived at the conclusion that the beds containing *Pyrotherium* were not only later than these, but probably more recent even than the Marine Patagonian Formation itself. His words are:—"It is certainly remarkable that in these beds containing Dinosaurian remains, associated, according to Ameghino, with the remains of mammals, some of them, as for example *Pyrotherium*, of immense size, only a little less than that of the elephant and consequently easily to be seen, I could have searched for weeks without ever finding a single mammalian bone, while every day I found Dinosaurian remains."

We await Dr Ameghino's observations on Mr Hatcher's results with great interest, for on the settlement of the Patagonian problem great issues depend. It is to be hoped that ere long some other geologist skilled in the modern methods of stratigraphy will investigate the subject and give us another independent opinion.

The Aepyornis of Madagascar

We have several times referred to the important results of Dr Forsyth Major's explorations and researches in Madagascar. We now have the pleasure of directing attention to the latest fruit of his labours in the form of a nearly complete skeleton of the extinct struthious bird, *Aepyornis*, which was mounted for exhibition last month in the public galleries of the British Museum (Natural History), South Kensington. The skeleton is shown in the accompanying photograph (Plate X.), for which we are indebted to the courtesy of the Editor of the *Geological Magazine*, and it was described last June in the journal just mentioned by Mr C. W. Andrews. The bird must have been about five feet in height when alive, so that it represents one of the smaller species of the genus. The bones do not belong to one and the same individual, but they have been selected from a very large series and have every appearance of giving the animal its correct proportions. The skull is imperfect in front, but the top of the brain-case is marked with rows of deep pits, which appear to indicate the original presence of a crest of large feathers. The mandible is very stout. The vertebral column, as reconstructed, consists of twenty true cervicals and eight vertebrae bearing free ribs; the fused pelvic vertebrae are about twenty in

number. The sternum, or breast bone, is very short and broad, while the coraco-scapula is much like that of a cassowary. The wing is reduced to a mere rudiment, namely a small humerus and one other bone which seems to represent the whole of the rest of the limb. The hind-limb has three toes, and there also seems to have been a rudimentary hallux, though this has not yet been found. The specimen in the Museum is appropriately placed by the side of *Dinornis* and the ostrich, and is one of the most striking additions to the collection of fossil birds acquired during recent years.

Swedish Arctic Exploration

We are very glad to learn that the Swedish Arctic expedition, which Prof. A. G. Nathorst has been advocating for some time, is likely to take definite shape next year. The necessary means have at last been procured, thanks chiefly to the liberality of King Oscar and the late Baron Oscar Dickson. The main objects of the expedition, Prof. Nathorst informs us, are the scientific, and especially the geological, investigation of the east coast of Spitzbergen, which as yet is very little known; the charting and exploration of Kung Karl's Land; and, should time permit, of the little known islands between Spitzbergen and Franz Josef Land. It is proposed to take an observer specially familiar with the study of recent land movements and glacial action, a botanist, two zoologists, who amongst other things will, together with the hydrographer, make observations on the plankton; the hydrographer will also be a meteorologist. In addition there will be a combined cartographer and photographer, and a specialist in degree-measurement. An arc of the meridian will be carefully measured on the east coast of Spitzbergen, but during the course of one summer it will only be possible to make the preliminary studies for this work. Professor Nathorst himself will continue his previous studies in both the botany and geology of the region, as well as having the command of the expedition. He hopes to be able to take three scientific helpers in addition to those mentioned, but this will depend on the size of the vessel that is obtained.

With the notable exception of Andree's bold attempt, Sweden has of late not taken her usual share in Polar exploration, but the names of Torell, Loven, Nordenskiöld, and other Swedes are so intimately connected with Spitzbergen that the forthcoming expedition is but the natural sequel to a long series of scientific voyages. With the fresh forces now at his command for attacking the problems of the Arctic seas, Professor Nathorst may expect, and we sincerely hope that he will meet with, even more success than his predecessors.

American Fossil Brachiopoda

Despite the many valuable monographs that have been issued of late years by our American colleagues, the study of the fossil Brachiopoda has hitherto been a task to be undertaken with trepidation. The labours of James Hall, J. M. Clarke, C. E. Beecher, and Charles Schuchert have considerably changed our views as to the inter-relationship and classification of these animals, and have rendered necessary extensive revision of nomenclature. But while we had an uneasy feeling that the names in our text-books and the labels in our museums were of too ancient a kind, we shrank from the difficult duty of resorting and renaming. The magician prepared to substitute new lamps for old arises in the person of Mr Charles Schuchert, who essays the task for the American fossil species; and, since North America seems to have been the gathering-place of the brachiopod clans in Palaeozoic times, much of this welcome light is also available for European species.

The book that forms the necessary keystone to previous writings is entitled "A Synopsis of American fossil Brachiopoda, including bibliography and synonymy," and has just been issued from Washington as *Bulletin, No.* 87, of the United States Geological Survey. It has, however, been prepared after official hours, and represents the work of eleven years. The main part of the book is the "Index and Bibliography of American fossil Brachiopoda;" which occupies 227 pages and contains about 10,000 references. All names that have ever been applied, rightly or wrongly, to fossil brachiopods of North and South America, are here given in alphabetical order. The names accepted by the author, after careful research, as valid, are printed in bolder type, and under each is given the geological age, chief localities, and a list of synonyms. Under each generic name is quoted the name of the species, whether American or not, that served as the original type of the genus; Mr Schuchert calls this the 'genotype.'

The index is preceded by some useful and interesting chapters on general questions, accompanied by some elaborate tables. These chapters are:—"I. Geologic development and geographic distribution of American fossil Brachiopoda." "II. Brachiopod terminology, applied to fossil forms"—practically an alphabetical glossary of terms. "III. Biologic development of the Brachiopoda"—an exceedingly important chapter. "IV. Morphology of the Brachia," contributed by Dr C. E. Beecher. "V. Classification of the Brachiopoda," in which the point of chief importance is the entire dismissal of the old division into Lyopomata and Arthropomata (= Inarticulata and Articulata), as discordant with the facts of race-development.

While this book will, as we have said, be an important aid to students of the European fossil brachiopods, it by no means fills the gap that has long yawned before them. A corresponding European work is greatly to be desired. F. Béclard began something of the kind, but death has stopped his energetic research. In England we hardly take our fair share in the production of these useful bibliographic lists of recent or fossil organisms; and yet their publication would prevent many of the descriptions of supposed new species that are constantly being thrust upon us. Once upon a time there was a great flourish of trumpets over a new edition of Morris' "Catalogue of British Fossils" as a memorial to that geologist, but though the talkers were many the labourers were few. Surely the scheme might be undertaken by half a dozen really serious workers, and brought up to the end of the century.

To return to Mr Schuchert. We can do no more than thank him for the result of his labours: he has given us a key to the writings of the last two decades, he has simplified our researches, and has taken a heavy load off our minds. We congratulate him on the completion of his task, and are glad to place another solid contribution to zoological literature on our bookshelves.

Wasps and Weather

On two previous occasions (*Natural Science*, Vol. iii., pp. 273-275, and vi., pp. 178-179) Mr Oswald H. Latter has called attention in these pages to an apparent connection between abundance or scarcity of wasps and certain meteorological phenomena. Briefly, his conclusion then was—(1) that wasps were favoured by dry springs and early summer, while if these seasons were wet wasps were scarce in the later summer and autumn; (2) that low temperature during the winter and early spring had little or no effect. He now favours us with a supplementary communication, pointing out how his observations and enquiries during the present year confirm his earlier conclusions. "A complete survey," he remarks, "must begin with September 1896: this was the wettest September known for many years, the rainfall at Godalming amounting to 7·12 inches; from that month up to the end of May of the present year the rainfall for every month, except November, was considerably in excess of the average, while that of March amounted to 5·01 inches. The total rainfall for the nine months, September 1896 to May 1897, amounted to 30·38 inches, which is rather more than 2½ inches in excess of the annual rainfall. Thus the period of hibernation and nest-founding among wasps was extremely rainy. The temperature of these months, notably that of February and March, was decidedly

above the average, and thus many Queens were early tempted from their winter quarters. Turning now to the numbers of wasps observed—in spring there were many 'Queens' to be seen, and many persons observed to me that we might expect a recurrence of the 'plague' of 1893. This prediction was entirely falsified, for in all parts of the country wasps were conspicuously absent during August and September. My own observations on this point were conducted in Surrey, Hampshire, Norfolk, Hertfordshire, and Kent, and I am informed by friends that the same was noticeable in Scotland, Lancashire, and Somersetshire. These facts seem to me sufficiently conclusive of the truth of my former conclusion, and I should esteem it a favour to be allowed to invite information from any of your readers whose experience may perhaps, during the past year, have furnished further evidence in the same or the opposite direction."

Models of Cells

Prof. A. L. Herrera has recently published in the *Memorias y Revista de la Sociedad Cientifica 'Antonio Alzate,'* Mexico, 1897, two interesting essays, in which he describes some attempts of his to make working models of the impact of forces upon cells and protoplasm. He points out that in the part of physiology dealing with the elaborate mechanisms of higher animals the construction of models, such as those to illustrate the flight of insects or the action of the valves of the heart, has been useful; and he attempts to apply the same principle to the fundamental phenomena of protoplasm. To a certain extent he has been anticipated by Bütschli and others, and we are bound to admit that his working models are of coarser texture and apparently less adapted to the delicate reaction of protoplasm than the oil foams of his predecessors. None the less, many of his experiments are interesting and ingenious, and may serve a useful purpose in kindergarten science. In the first essay, entitled 'Los Infusorios Artificiales,' he tries to explain vibratile movements of cilia by means of elastic tubes containing diffusible liquids and placed in other liquids. His idea appears to be that osmosis currents between protoplasm and water and the stresses produced in the elastic cell-wall set up the vibrations. In the second essay, written in French, he describes a series of experiments showing the reaction of elastic spherical bodies to pressure by the elastic surfaces. He obtained a number of results strikingly resembling known animal and plant forms. We commend his essays to the curious.

A New Scientific Serial from Jamaica

As we briefly announced last month (p. 351), Jamaica furnishes us with one more promising infant in an over-populated world of scientific literature! To such a new-born child it can scarcely be said, "weeping thou sat'st while all around thee smiled," for, while the infant chuckles, distracted naturalists shed inky tears. It might sound rude to say that all such babes must come to the workhouse. To the house in which the specialist works sooner or later they have to come. If every name in the atlas of the world insists upon having its own separate representative in serial scientific literature, the wasteful dissipation of energy will increase in a lamentable degree. The diligence of the student will be more and more exhausted in a vain attempt to garner all the scattered fragments of information, which may or may not be of value, concerning each strictly limited branch of enquiry. It is, therefore, only with a moderate rapture of welcome that we can greet this first number of the *Annals of the Institute of Jamaica*.

The opening number is entirely devoted to a list of crustaceans. Faunistic catalogues are not unfrequently a weary waste of misapplied industry. They often contain no guarantee whatever that the author knows what he is writing about. When the identifications are original, they are as likely as not to be wrong; when they are borrowed, they are not very unlikely to be the endorsement of some ancient error. Miss Rathbun's "List of the Decapod Crustacea of Jamaica" stands on a different footing, because she happens to combine with a very exact knowledge of the objects catalogued a full and accurate acquaintance with the literature of the subject. The list, therefore, is a critical list, and great confidence may be placed in the names and synonyms and geographical distribution of species which it records. But it also contains notes and descriptions of independent importance. Some of these are quoted at full length, though without marks of quotation, from earlier papers, while others contain corrections of previously published opinions. Surely in the interests of science the repetitions would have been better omitted, and still more surely in the interests of science the corrections would have better appeared in the *Proceedings* which published the original statements.

While placing the highest value on Miss Rathbun's knowledge and acumen, we cannot always accept her decisions on points of nomenclature. The name *Stenorynchus seticornis* (Herbst) should stand, whether Slabber were right or wrong in stating that his specimen came from the East Indies. He appears to have kept his East and West Indian crabs together, and may have made some confusion, and if not, as the present list shows, the same species of

crab may occur in both the Indies. In naming *Uca heterochela* (Lamarck), the change from Lamarck's more scholarly *heterochelos* is unneeded, since the Greek ἑτερόχηλος applies equally to the masculine and feminine genders. In dealing with the genus *Palaemon*, Miss Rathbun unhappily relies on Latreille's 'Considerations générales' of 1810, a book with a long-winded title too troublesome to quote, a book crowded with definitions that don't define, and ending with a list of types that are not described.

A Year-Book of Agriculture

The Year-Book of the U.S. Department of Agriculture for 1896 has just reached us, and set us wondering why we have not a Department of Agriculture manned by scientific experts, which might issue each year for the benefit of farmers and others interested in the subject as much useful matter as is contained in the six hundred odd pages of the Transatlantic publication. The history of the year-book is this. It is the successor of the Agricultural Report which, in its original form, was made up almost wholly of business reports for the use of Congress. When, however, it began to circulate more freely among farmers, papers on agriculture, and discussions on the results of scientific investigations were introduced, and it gradually became more and more a popular report, business and executive matter being reduced to the smallest possible proportion, till finally it was decided (in 1895) to issue it in two parts—viz. (1) an executive and business report, (2) a volume of papers "specially suited to interest and instruct the farmers of the country," and to include also "a general report of the operations of the Department for their information." This second part is the Year-Book, and the one now before us is the third of the series. It is published in an edition of 500,000 copies for free distributing, and is therefore, as the assistant secretary remarks in the preface, "in many respects unique." Following the report of the Secretary, which occupies nearly fifty pages and is eminently suggestive, are thirty papers (filling 500 pages) contributed by nearly as many scientific expert members of the staff. The 164 figures and six plates are a useful addition. Thus the value of a paper on some common poisonous plants is much enhanced by very passable pictures of the poison ivy (*Rhusradicans*), water hemlock (*Cicuta maculata*), death cup (*Amanita phalloides*), and others. The same applies to some remarks by Mr Herbert Webber on the influence of environment on plant varieties. Enumeration of some of the titles will give an idea of the wide scope of the Year-Book:—extermination of noxious animals by bounties—potash and its function in agriculture—the country

slaughter-house as a factor in the spread of disease—the blue jay and its food—migration of weeds—diseases of shade and ornamental trees—care of dairy utensils—and finally one by M. E. Tisseraud, Councillor of State and Director of Agriculture of France, entitled, An Ideal Department of Agriculture and Industries, which is reprinted from a report of a House of Commons committee on the establishment of a Department of Agriculture and Industries for Ireland! An appendix of one hundred pages includes an account of the organisation of the Department of Agriculture, copious statistics of crops, exports, imports, and numerous items of useful information.

An Ideal Agriculture Department

We make no apology for remarks on Agriculture, which is only practical Natural Science on a large scale, pursued for the best possible object—the benefit of the race. In the paper to which we have just referred M. Tisseraud raises certain points which are worthy of emphasis. The agriculture of Europe, like an old and leaking ship, tossed and buffeted on a sea of breakers, needs, to save it from foundering, to be steered by abler hands and navigated by pilots who will join to a thorough practical training a profound and extensive knowledge—scientific knowledge. Hence the need for a Ministry of Agriculture; not a sort of Providence on which all may lean, and which by a series of miracles can supply remedies for all evils, nor a central authority which shall absorb all services and assume the functions of private individuals and voluntary associations, but an authority which shall tend to awaken the spirit of initiative and independence, and to stimulate and develop it among the agriculturists themselves. M. Tisseraud refers to the methods of control, in France, where by co-operation with local societies and associations, the encouragement of private agricultural schools, and the establishment, with the assistance of skilful farmers, of from 3000 to 4000 'champs de demonstration' each year, the State is able to accomplish an enormous amount of good with a very little expenditure.

Another task of a ministry of agriculture is to induce the most eminent scientists to occupy themselves with agricultural questions. It "must not be niggardly in its encouragements and subsidies to such men, for their discoveries will repay, with large interest, the expenditure which may have been incurred to enable them to carry on their researches." By this means there will be prepared for agriculture "an élite of men to direct it in the way of progress and of the application of science." The Minister, in the nature of things as at present constituted, must be a politician, but not so

the men at the head of the different divisions of the department, who should unite to a great experience of administration profound technical knowledge and an incontestable authority in the questions with which they have to deal, and whose office should be of a permanent character. Such a director must have the choosing of the staff placed under his orders. He must be their master, and be able to reward those who show merit and zeal, and to remove or punish those who cannot or do not properly discharge their duties. "Let the director himself be absolutely responsible for the good working of his department, and let him be replaced if he proves inefficient." Finally, M. Tisseraud suggests the appointment of scientific counsellors selected from the most distinguished agriculturists and men of science, and technical committees of professional men, specialists, and practical experts, from whom the Minister may obtain "trustworthy advice and indispensable light for rightly seeing, and judging, and forming in full security the decisions which concern the department over which he presides."

Catalogue of Fibre Plants

Another excellent specimen of the work of the United States Department of Agriculture comes to hand almost while writing the above. It is a descriptive list of useful fibre plants of the world by C. R. Dodge, special agent in charge of fibre investigations. More than a thousand kinds are enumerated under the botanical names of the plants producing them, while common and native names are also included in the alphabetical arrangement. The book is a large octavo of 360 pages; there are more than one hundred figures in the text, and thirteen excellent photographic plates showing the habits of the various plants. Much information is given about the more important kinds, including the structure and properties of the actual fibre, the source and method of cultivation of the plant, the preparation of the fibre, and references to the literature. The great number of native names possessed by some species is evidence of their long use. New Zealand flax (*Phormium tenax*), a liliaceous plant, has nearly sixty native names. Captain Cook, who first brought it to the notice of Europeans, found it in common use among the aboriginal New Zealanders; he speaks of it as "a grass plant like flags, the nature of flax or hemp, but superior in quality to either, of which the natives make clothing, lines, etc." Mr Dodge gives in a useful introduction a general account of the history, chemistry, and structure of fibres, and suggests also a classification. His system of arrangement serves to show what very various parts of the plant are used for the purpose; for it includes not only true

fibres derived from the wood or bast tissue respectively, but whole stems, roots or leaves, or split and shredded leaves, as well as external hair-like growths, such as cotton, and also a class of "pseudo-fibres." The last comprises certain mosses, like *Sphagnum*, used for packing, seaweed wrought into lines or cordage, and the mycelium of certain fungi. The book will be a welcome addition to the libraries of individuals and institutions interested in economic botany. To enhance the value of future editions, the author requests notes or further information on any fibre plants, and especially photographs of foreign species.

Wing Neuration in the Lepidoptera

Mr A. Radcliffe Grote has been insisting both in German (*Ill. Wochenschr. f. Entomologie*, band II., no. 28), and in English (*Entom. Record*, vol. ix., no. 10) on the advantage of employing photography in the illustration of the wing-nervures of moths and butterflies, as by this means only can absolute accuracy be ensured. British entomologists will be specially interested by Mr Grote's severe strictures on Mr Meyrick's descriptions and drawings of wing-neuration in his revision of the Geometridae (*Trans. Ent. Soc.*, 1892) and his recent "Handbook of British Lepidoptera." Comparison of Mr Meyrick's figures with the photographs have led Mr Grote to the conclusion that in the former "the distances, relative direction, and at times the point of origin are frequently all wrong. Worse than this, Mr Meyrick supplies nervures which have no existence . . . and omits nervures . . . which are distinct in nature." We await with interest Mr Meyrick's reply to this criticism.

I

The Problems of the British Fauna

IN the current volume of *Natural Science* (pp. 223-4) appeared a short editorial comment on my friend Dr R. F. Scharff's paper on the Origin of the European Fauna.[1] The Editor has misunderstood Dr Scharff's views in several particulars, and has nevertheless expressed the fear that his "speculations will prejudice the use of zoological distribution in geological investigations." As the problems raised are of great interest to all naturalists, a further examination of the subject may perhaps be allowed. It is somewhat unfortunate that the present writer approaches the question from the same standpoint as that of Dr Scharff—zoological geography. But it is to be hoped that some of the special students of our Pliocene and Pleistocene deposits will, in due course, favour us with their criticism.

The problems suggested by the fauna and flora of the British Islands appeal in a marked degree to naturalists who live in Ireland, especially if those naturalists happen to be English immigrants to the sister isle. A botanist or a zoologist who has grown up in the south of England, and has then transferred himself to Ireland, is struck by the absence of many of his familiar wild friends, and the presence of many forms of life hitherto unknown to him as British species. The peculiarities of the Irish flora, such as the occurrence of Pyrenean saxifrages and Mediterranean heaths in western Ireland, have long been familiar to naturalists, and are discussed in the classical memoir of Forbes.[2] It may be well, however, to recall a few of the corresponding facts regarding the fauna. The student of vertebrates notices the absence, for example, from Ireland of the Common Hare (*Lepus europaeus*), the Voles, the Mole, the Weasel, the Polecat, the Nightingale, and all reptiles except the Viviparous Lizard. The entomologist misses such conspicuous insects as the Stag-beetle (*Lucanus cervus*), the great water beetle *Hydrophilus piceus*, and the Large tortoiseshell butterfly (*Vanessa polychloros*). These are representatives of a group of animals to which the present writer has applied the term "Teutonic fauna,"[3] while Dr Scharff, in his recent

[1] *Proc. R. Irish Acad.* (3), vol. iv., pp. 427-514. An excellent summary appeared in *Nature* of October 28th, 1897.
[2] *Mem. Geol. Survey Gt. Britain*, vol. i., 1846.
[3] *Museums Assocn. Report*, 1894.

paper, designates the mammals among them as distinctively 'Eastern' or 'Siberian.' The absence of these Siberian mammals from Ireland may perhaps be regarded as the central fact on which his views concerning the British fauna are founded. Most of the animals of this group die out in Great Britain as one travels north or west. It must be specially noted, however, that the mammals range over the greater part of the island. The Common Hare extends from Cornwall to the shores of the Pentland Firth; the Weasel and the Viper range far north into Scotland. But most of the corresponding invertebrates are not found north of the Trent or west of the Severn.

The naturalist in Ireland is compensated for the loss of this eastern fauna by the presence of two most interesting and distinct sets of animals, almost unrepresented in the south-east of England. It has been mentioned that the Common Hare is absent from Ireland, but the Varying Hare (*Lepus variabilis*) occurs all over the country, from north to south, both on the hills and in the plain. This is a typically arctic and alpine animal, with a complete circumpolar range, confined in Great Britain to the Highlands of Scotland. Quite a number of insects, which in Great Britain are to be found only in the north range to the extreme south of Ireland, such as the marsh ringlet butterfly (*Coenonympha typhon*) and the ground-beetles *Carabus clathratus* and *C. glabratus*. But perhaps the most striking example of this northern fauna is the ground-beetle *Pelophila borealis*, which has been found in most of the northern and western counties of Ireland, from Antrim to Kerry in the far south-west. This beetle is, so far, unknown on the mainland of Great Britain, but it occurs in the Orkneys; on the continent it is an inhabitant of high northern latitudes. Together with this arctic and alpine group may be mentioned the three species of North American fresh-water sponges, *Ephydatia crateriformis*, *Heteromeyenia Ryderi*, and *Tubella pennsylvanica*, which Dr Hanitsch[1] has lately described from lakes in western Ireland. These are comparable to the few North American plants which grow wild in the same districts. One or two of the plants have Scotch stations; but both plants and sponges are unknown on the continent of Europe.

The second characteristic group of the Irish fauna—like the peculiar plants of the western counties, the Arbutus, London Pride, and St Dabeoc's Heath—shows striking affinity with the life of south-western Europe and the Mediterranean region. Forbes, in his memoir already referred to, expressed the opinion that no fauna corresponding to this Hibernian flora exists in the British Isles. Everyone, however, agreed in assigning to this type the Portuguese slug, *Geomalacus maculosus*, when it was discovered spread over a small area in counties Cork and Kerry. Recently a number of

[1] *Irish Nat.*, vol. iv., 1895, pp. 122-131.

animals have been recognised which undoubtedly show similar faunistic relationships. Some of these are new discoveries, and are apparently confined to Ireland, such as the millipede *Polydesmus gallicus*,[1] the earthworms *Allolobophora veneta* and *A. georgii*,[2] and the weevil *Otiorrhynchus auropunctatus*.[3] Others have long been known as British animals, and their occurrence in the west of Great Britain as well as in Ireland has probably caused their faunistic import to be overlooked. Such are the snail *Helix pisana*, the wood-louse *Platyarthrus hoffmanseggii*, the ground beetle *Eurynebria complanata*, and the weevil *Mesites tardyi*. Some of these animals are found both in the east and west of Ireland, others only on the east coast. In Great Britain they occur mostly in the south-west, but the last-named is an example of a section which ranges northward into western Scotland. Abroad all are characteristic of southern and south-western Europe, while several are found in the Azores, Madeira, and Canary Islands. It is most important to take the foreign range into consideration when assigning animals to a distributional type. Just as the Common Hare is spread far to the north in Great Britain, as compared with many other members of the 'Siberian' fauna, so a number of animals belonging to the South-western fauna range farther to the east than the more typical species of the group. For instance, the slugs of the genus *Testacella* must be referred to the South-western section, when we consider the general range of the genus, though some species occur in our eastern counties. And Dr Scharff would add to this fauna many of our widely distributed species—the Bullfinch among birds, for example.

He believes moreover that this South-western fauna merges gradually into a 'South-central' fauna, including the Badger among mammals, and *Helix virgata*, *H. acuta*, and *H. nemoralis* among snails. And to these combined southern groups he is inclined to ascribe the bulk of the Irish animals, even those with a wide range both in Ireland and in Great Britain; except a few species that may have invaded the country since it became an island—such as the white butterflies, *Pieris brassicae* and *P. rapae*, and the 'painted lady,' *Pyrameis cardui*, or that have been apparently introduced by man—such as the rats and the house mouse.

A few remarks are necessary regarding the relative ages which are to be ascribed to these different sections of the British fauna. The South-western group, the most typical members of which are found in the most remote parts of the country, with ranges often discontinuous, are evidently the oldest. The 'Siberian' animals, which

[1] R. I. Pocock, *Irish Nat.*, vol. ii., pp. 309-312. [2] H. Friend, *id.*, vol. v., pp. 69-73.
[3] G. H. Carpenter, *id.*, vol. iv., pp. 213-218.

are confined as a rule to eastern and south-eastern England, and none of which have been able to reach Ireland, are clearly the newest. The Northern animals must therefore come between these two in regard to the time when they entered our area. It will be remembered that Forbes, when discussing the distributional groups of British plants, regarded the most western ('Hibernian') flora as the oldest, the 'Germanic' flora as the newest, and the arctic and alpine flora as of intermediate age. Forbes, however, considered the plants of general British distribution to have entered the country subsequently to the arctic and alpine species. And as he observed that there is a gradual transition from the most typical 'Germanic' to the most widely-spread 'British' type, he regarded all the immigrants since the Northern flora—that is to say the 'British,' 'English' and 'Germanic' types of Watson,[1] as belonging to one great central European flora, some of whose members have spread much more widely in our islands than have others. Forbes, moreover, separated two small groups of plants, one typical of Cornwall and Devon ('Norman' flora), the other characteristic of the chalk districts of south-eastern England ('Kentish' flora), which he believed to be entirely distinct from the recent Germanic flora. To these small sections he ascribed an age between that of the South-western and that of the Northern flora.

Dr Scharff's estimate of the relative ages of the sections of the British fauna differs from Forbes' view of the ages of the corresponding sections of the flora in one important particular. While Forbes placed the bulk of our widespread plants later than the arctic and alpine species, Dr Scharff considers that—at least as regards the species found in Ireland—the vast majority of the animals are of southern origin, and not more recent than the arctic and alpine species. As mentioned above, he believes that there is a gradual transition from animals of the most typical 'Hibernian' type, such as *Geomalacus maculosus*, to such widespread animals of his 'South-central' group as the Badger and the Fox.

The question of the exact geological period during which each section of the fauna entered the British area, and by what route the animals reached our territory, must now be considered. With regard to the flora, Forbes believed that the Hibernian plants lived on a now sunken Atlantis in Miocene times, and reached their present Irish and Iberian stations from the west before the Ice Age. The Cornish and Norman floras were supposed to have come into the country from the south-west or south—of course across the dry area of the Channel—also before the Ice Age. The arctic and alpine plants, Forbes naturally thought to be the relics of the Glacial Period itself. And he believed the rest of the British flora—the

[1] "Cybele Britannica," London, 1870.

'British,' 'English' and 'Germanic' types—to have entered the country from the east and south-east across the dry area of the North Sea and the Straits of Dover during the subsequent period when the British territory had emerged from the Glacial sea, England being united to the Continent, and Ireland to Great Britain.

At the time when Forbes wrote, the glacial deposits were believed to have been laid down on the bed of a sea covered with floating ice. The subsequent adoption by the majority of geologists of the theory that the Boulder Clay represents the ground moraine of vast sheets of land ice has led most recent writers on the British fauna and flora to regard most if not the whole of the living things in our area as post-glacial immigrants. Whatever animals and plants lived in these islands during Pliocene times are presumed by Professor James Geikie, and those who share his views, to have been exterminated by the terrible rigour of the glacial conditions during the Pleistocene age. And the general view at present is that it was not until the climate improved in later Pleistocene times that the country again became the abode of animal and vegetable life. On this theory it would seem certain that the arctic and alpine species were the first to establish themselves in our area.

Now, the results to which Dr Scharff's studies have led him are in startling opposition to the current opinion just mentioned. He believes that, with the exception of the 'Siberian' section, the whole of the British fauna entered the country in Pliocene or the earliest Pleistocene times. With regard specially to the Irish fauna, he considers that all the animals which now inhabit Ireland must have passed into that island in the Pliocene, or, at latest, about the opening of the Pleistocene period, there being, in his view, no evidence of any land-connection between England and Ireland after that date. It is hardly necessary to recall the fact that the absence of so many British animals and plants from Ireland has led naturalists without exception to regard that country as an older island than Great Britain, whatever geological age they may ascribe to the fauna and flora.

It is specially the study of the past and present distribution of the British mammals that has led Dr Scharff to his results. The 'Siberian' mammals which are found—living or extinct—in Great Britain, but not in Ireland, furnish, as has been said, the key to his argument. Remains of these mammals, preserved in the continental Pleistocene deposits, enable the course of their migration from east to west to be traced in considerable detail. They lived in Siberia in Pliocene times, but in Europe their remains are not found except in beds later than the Lower Boulder Clay, which Dr Scharff suggests was laid down in the northern part of a sea connecting the

Caspian and Aral with the White Sea and the Baltic, thus forming an effectual barrier to the westward course of the mammals. The existence of such a sea is supported by the presence of arctic forms of life in the Caspian, and the occurrence of the Caspian mollusc *Dreyssensia polymorpha* in the Lower Boulder Clay of Germany. As this central European sea was replaced in part by a land surface, the way was opened for the Siberian mammals to pass on into western Europe. Now we are confronted with the startling fact that the British deposit in which these mammals first appear is the Forest Bed, usually considered the newest member of the Pliocene series. Are we to suppose, Dr Scharff asks, that the animals made their way into England by Asia Minor, Greece and Southern Europe, and so reached our shores before Central Europe was open to them? That part of the older southern fauna—the 'South-Central' section—travelled into Western Europe by this route from Siberia during Pliocene times he does believe. But, he argues, it is impossible that the true 'Siberian' animals could have passed that way, seeing that their remains are entirely absent from South European, as well as from Irish, Scottish and Scandinavian, deposits. He is therefore driven to the conclusion that the Forest Bed and other British deposits usually classed as Newer Pliocene must be considered as rather later than the Lower Continental Boulder Clay, and reckoned to be of Pleistocene age. In support of this correlation he also brings forward the presence of arctic shells in the newer crags.[1]

Having thus fixed the period when these Siberian mammals appeared in England, Dr Scharff believes that he has obtained the latest possible date for the 'last link' of the land-connection between England and Ireland. For if the way into Ireland remained open long after these mammals reached English territory, what can have prevented their onward course to the western island? The wide range of the mammals as compared with the restricted range of the invertebrates of the same faunistic section has been dwelt upon in the opening part of this paper. It is certain that the vast number of widespread invertebrates that inhabit Ireland as well as Great Britain must have passed over the Irish Sea when it was a lake and river valley, or crossed the later northern isthmus which joined northern Ireland to south-western Scotland. But as the Siberian mammals were kept out of Scotland by the Pleistocene sea, this northern isthmus may be left out of reckoning as far as they are concerned. If the slowly-moving army of spiders, beetles, snails

[1] The reader is referred to Dr Scharff's paper for the numerous references supporting these positions. It will be seen that the editorial statements of Dr Scharff's views (*supra*, p. 224, "that the lower continental boulder clay is Pliocene . . . that the Siberian mammals migrated into Western Europe to the south of this sea") convey the exact reverse of the opinions really advocated by the author.

and slugs did not invade Ireland until after the Siberian mammals were in England, why were the latter unable to reach Ireland as well? Such, briefly, is Dr Scharff's argument for the pre-Glacial immigration of the Irish fauna.

Turning to the Northern section of the fauna, Dr Scharff argues that it must have entered Scotland by a land-connection from Scandinavia, and so passed southwards into Ireland. This land-connection he believes, in common with most geologists, to have been continued northwards to Spitzbergen, and westward to Greenland and North America. Thus a way was open for animals with a circumpolar range to wander southwards, while North American forms were able to invade Western Europe. The continuous coast-line to the north of the Atlantic, shutting off that ocean from the Arctic Sea, must have ensured a mild climate to its waters and shores. The vast majority of geologists would, of course, regard this land-connection and the migrations which passed over it as post-Glacial. Dr Scharff, necessarily considering the northern fauna older than the Siberian, believes, on the contrary, that its entry into our area must be put back to the time when the ice-laden sea of the Lower Boulder Clay covered Central Europe and the newer crags were being laid down in eastern England. The land-connection between Scandinavia and Ireland he considers, however, to have persisted into late Pleistocene times.

It has already been mentioned that the bulk of the Irish fauna is supposed by Dr Scharff to have come from South-western and South-central Europe, and that the more western section is regarded by him (as the corresponding section of the flora was regarded by Forbes) as the oldest section of the whole British fauna. Dr Scharff does not share Forbes' view of an extensive Atlantic continent; he believes that a western continental coast-line, including, of course, a tract to the west of the present British and Irish area, meets all the requirements of the facts. Across the valleys which occupied the present beds of the English and St George's Channels the animals of these southern migrations passed into Great Britain and Ireland; according to Dr Scharff, through the Pliocene and up to the earliest Pleistocene period. But the land-connection between Great Britain and Ireland broke down in the south sooner than in the north, so that the arctic migration could go on after the southern migrations had been cut off. Some of the animals of the 'South-central' migration are traced by Dr Scharff back to Siberia, where he believes they originated. He points out, moreover, that the same species can sometimes be proved to have taken part both in the 'South-central' and in the (later) true 'Siberian' migration. In such cases, however, a distinct race of the species usually characterises each migration. For example, the Irish race of the Red Deer is the

small-antlered form which can be traced, by its remains in south European beds, from Western Asia into Greece, and "along the borders of the Mediterranean, at the time when Corsica and Sardinia were still connected with Sicily and Greece on the one hand and with Tunis on the other." In this way it is suggested that animals from western Asia and south-eastern Europe found their way to the western edge of the continent, while the central European plain was still covered by sea.

If Dr Scharff's views as to the geological periods during which the British fauna entered the country be accepted, it follows that the vast majority of our animal population must have survived the rigours of the Ice Age; as regards Ireland, the whole fauna (except comparatively modern immigrants) must have lived in the area from a time before the deposition of the British Lower Boulder Clay. It will be remembered that Forbes, who believed the distinctive South-western flora to be pre-glacial, suggested that the plants survived in a sunken land to the south-west. Dr Scharff, however, rejects the idea of such an asylum for the fauna on the ground that the south-western corner of Ireland is remarkably poor in species, many forms of life, common throughout the rest of the island, being absent from the peninsulas of counties Cork and Kerry; for example, the Helices of the sub-genus *Xerophila*. He insists that portions at least of the present Irish area must have been able to support the present animal population throughout the Pleistocene period.

Those geologists who adopt the extreme view of the glaciation of Ireland, advocated by the Rev. M. H. Close, and accepted by Professor Hull,[1] will naturally reject Dr Scharff's conclusions with decision, if not with derision. For, according to the opinion of this school, an ice-sheet of great depth covered the whole country. It is needless to say that Dr Scharff rejects with equal decision the existence of such an ice-sheet. In the closing section of his paper, he expresses his agreement with those geologists who believe that the Boulder Clay was formed in an ice-laden sea, and. not as the ground-moraine of vast glaciers. Of course, this view requires the submergence of much of the country. But, recalling the opinion of several geologists that the western margin of the British area stood higher in relation to the eastern during the Glacial Period than now, Dr Scharff reconstructs the physical geography of our islands during that time of greatest submergence, which left shell-bearing gravels on the Dublin mountains and Moel Tryfaen. According to his map, the Scottish highlands, the Hebrides, and northern, western, and southern Ireland formed a peninsula still continuous with Scandinavia; the Scottish lowlands and northern England were an

[1] "The Physical Geology and Geography of Ireland," London, 1878.

archipelago; central and southern Wales an island; while the south and midlands of England were joined to France by an isthmus. The sea covered nearly the whole of eastern England, and stretched across north Wales, and over eastern and central Ireland. Zoological evidence for this transgression of the northern sea over eastern Ireland is found in the distribution of the arctic marine crustacean *Mysis relicta*, which forms part of the 'relic fauna' not only of the Swedish lakes, but also of Lough Neagh. The sea separating southern and central England from Scotland, as well as from Ireland, checked the northern as well as the western progress of the 'Siberian' mammals. None of these animals are found fossil in Scottish Pleistocene deposits, though the recession of the glacial sea has in recent times opened a way to the north, of which the surviving species have availed themselves. But meanwhile the isthmus between Scotland and Ireland had become broken through.

Having thus put forward a summary of Dr Scharff's views as to the ages and paths of migration of the various sections of the British fauna, I venture, with some diffidence, to offer a few observations and suggestions. I entirely agree with Dr Scharff in considering the South-western as the oldest section of our fauna, and I have no doubt that it came into our area long before the Glacial Period. The North American plants and animals seem to me to be more ancient than Dr Scharff is inclined to admit. He classes them with the general Northern fauna, but I believe that their very restricted and discontinuous ranges along the extreme western margin of Europe mark them as decidedly older than those northern animals and plants which have a general circumpolar distribution.

Study of the distribution of British insects shows that there is a Southern fauna[1] distinct from the South-western, in that its members occur generally in southern Britain, as well as in Ireland and western Britain, and have a wide continental range. It is clearly newer than the South-western fauna, yet the fact that it is confined in Ireland to the south and west suggests that it is of considerable geological age. Along the west coast of Ireland the insects of this group often range some distance to the north, and their general British distribution around the west and south of our islands renders it likely that they held the country west and south of the area where the Glacial deposits were being formed, and have, since the retrocession of the agent which produced those deposits, been unable to spread far eastwards in Ireland or northwards in Great Britain. This fauna may safely be regarded as comparable to Forbes' Norman and Kentish floras, and older than the Arctic fauna. As yet, however, I am not prepared to accept so great an antiquity for the bulk of

[1] Examples of this group are the ground-beetles, *Carabus cancellatus* and *Panagaeus crux-major*, the butterflies *Gonepteryx rhamni*, and *Leptida sinapis*, and the moths *Zeuzera pyrina* and *Stauropus fagi*.

the widespread Irish animals as Dr Scharff claims by referring them to his 'South-central' group.

I quite agree with Dr Scharff in rejecting the theory that the whole of our fauna is post-Glacial, since that theory would require us to regard the Arctic animals as the oldest, whereas the distributional facts require us to consider the South-western section the oldest. But it seems to me that we are equally bound to consider the animals of the Northern fauna—restricted as they are to the hill regions and the west—as more ancient than the widespread species which form the dominant element in our fauna to-day. I am quite prepared to believe that many of these widespread species inhabited the southern part of our area throughout Pleistocene times, but it seems unlikely that they extended their range far to the north or west until the glacial conditions had passed away. Dr Scharff apparently believes that, the glacial deposits being due to a marine submergence, sufficiently extensive land tracts must have been left to enable the whole fauna to survive. But even many geologists who reject the theory that the Boulder Clay is a ground moraine, consider that the polished and scratched rock-surfaces beneath that deposit are evidences of a former extension of land-ice.

In the opening paragraphs of his paper, Dr Scharff makes the suggestive remark that the study of the fauna of a single island is the best starting-point for the study of a continental fauna. Hence he takes Ireland as the key to the greater problem of Europe. It seems likely that considerable light would be thrown on the special British problem by one of the smaller British islands, and I believe that in the Isle of Man we have evidence of a post-Glacial land-connection between Ireland and western England. Professor Carvill Lewis[1] and Mr Percy F. Kendall[2] found traces of glaciation up to the summit of the highest hills in the island, the former remarking that the whole shape of Snaefell is that of a 'roche moutonnée.' Whether we believe with these geologists that the 'Irish Sea glacier' passed over the summit of Snaefell, or prefer to consider the high-level drifts, boulders, and striated rock-surfaces as evidences of an ice-laden sea, it seems equally certain that the present inhabitants of Man must have reached that isle since the climax of the Glacial Period.

Now the fauna of the Isle of Man resembles on the whole that of Ireland, western England, and Wales. Its cliffs form the most northern station for certain species of moths, such as *Dianthoecia luteago* var. *barrettii*, *D. caesia* and *D. capsophila*, some of which are scattered along the western British and the eastern and southern Irish coasts as far as Land's End and Dingle Bay. If the Isle of Man could not have supported any fauna during the height of

[1] "Glacial Geology of Great Britain and Ireland," p. 357-9. [2] *Op. cit.*, pp. 433-4.

the Glacial Period, we are forced to the conclusion that its shores must, since then, have formed part of the northern coasts of a gulf opening to the south, down St George's Channel. As the climatic conditions improved, I believe that many animals of the old South-western fauna—such as *Helix pisana*, *Eurynebria complanata*, *Otiorrhynchus auropunctatus*—which had doubtless lived to the south of England and Ireland from Pliocene times, were able to make their way northwards along the shores of this ever-widening gulf to their present stations on the eastern Irish and western British shores. North of this gulf, I believe that the vast majority of our present widespread species passed from north-western England into Ireland, where they have spread from east to west. The difficulty raised by Dr Scharff that the 'Siberian' mammals were in England, and should have passed over to Ireland with the rest, is doubtless serious. But these mammals were kept out of Scotland until recent times, and they may well have been kept out of north-western England by an arm of the sea until the Irish land-connection had broken down. One of them, the English Hare, inhabits the Isle of Man, showing that the barrier which confined them to the east had been removed in time for that one species to spread so far, though not as far as to Ireland. The fact that the other mammals of the group—such as the Voles and the Mole—are absent from the Isle of Man proves that the Hare must have made the most of her chance to spread north-westward.

While, then, I find myself in almost complete agreement with Dr Scharff with regard to the older sections of our fauna, I think that those widespread species which survived the Glacial Period must have been confined to the more southern parts of our area, and have only subsequently spread northwards and westwards to Scotland and Ireland. Doubtless the speculations of the extreme glacial school regarding the total extinction of all life in our countries in Pleistocene times need revision in the light of the past and present distribution of species. At the same time there seems enough agreement among those who have specially studied the drift deposits to warn students of animal distribution that the conditions over much of the British Islands must have been unfavourable to the presence of a rich flora and fauna.

But in any case it seems to us a necessity to believe that a considerable proportion of the British flora and fauna did survive the Glacial Period in our area, or in the now submerged tracts adjacent thereto. Readers of *Natural Science* will doubtless recall Mr G. W. Bulman's paper,[1] in which a plea was entered for the pre-Glacial age of our animals and plants on the ground that no geological evidence of an elevation subsequent to the Ice Age could

[1] Vol. iii., pp. 261-6.

be brought forward. Neither of the rival glacial theories requires belief in the annihilation of all living things in our area. On the land-ice hypothesis there must have been a now submerged tract bordering on the Atlantic, and stretching beyond the present south coast of Ireland, which the late Professor Carvill Lewis recognised as an unglaciated area. A similar elevation to the west of our present British islands is believed to have accompanied the submergence by which the other school of geologists explain the Pleistocene deposits. And it is generally agreed that the south and, in part at least, the midland areas of England were free from glacial conditions. When we remember how distinctly temperate and even sub-tropical forms of life can be found to-day close to areas of glaciation, it must be admitted that there is no impossibility in the suggestion that the ancestors of the older plants and animals which we now see around us witnessed in our territory the coming and passing away of the age of ice.

GEO. H. CARPENTER.

II

The Provincial Museum

WHAT may fitly be called the provincial museum question has of late rapidly assumed considerable importance. The need for these institutions or the enthusiasm of their staff is not called in question; as a matter of fact, it is in a great measure to the energy of provincial curators, as manifested at meetings of the Museums' Association, that much of the present awakening is due. The evils from which these museums suffer are acknowledged to be a general lack of means, undermanning, and a partially or wholly untrained staff. The widespread attention which the subject receives even from the lay mind is evidence of a knowledge that better things are possible, and that a satisfactory settlement will tend to the public good and educational progress. The museum question has hitherto been attacked in two ways. The late Dr Goode in America, and Sir W. H. Flower in this country, have laboured alike to educate the public mind to the value and necessity of these institutions as factors in education, and to direct and stimulate museum workers. The second form of attack has been made by Professor Petrie (Brit. Assoc., Liverpool), and more recently by the editorial comments of *Natural Science* (Vol. xi., No. 66, Aug. 1897). It may be defined as the suggestion of remedial measures. The addresses and papers of Sir W. H. Flower and Dr Goode have undoubtedly done much to pave the way for better progress, but before the remedial measures which are now advocated can be made effective or adequate to the needs of provincial museums, it will be necessary to consider fully their present position as a whole.

Even a brief consideration of the provincial museum reveals much that is anomalous and unsatisfactory. Hardly any two can be said to work upon a common plan, whilst most develop and exist rather as the sport of circumstances than as the outcome of definite purpose and design. We much doubt if one can be pointed out which has an income at all equal to its needs, or which is able to develop and maintain its various sections according to their true value and proportion.

We find also that Government recognition is accorded to provincial museums in a vague and half-hearted manner. The Libraries and Museums Act can be put into operation if the people of a

borough demand it, but the operation of the act is dependent upon the action of the very people who are most needing instruction by museum agency. Not infrequently the act is put into operation to gain a library, and a museum of ill-assorted material grows up with it, by a system of accretion rather than assimilation. Indeed, it is this accretionary growth for want of a defined plan which is the bane of provincial museums, and one which the curators themselves are at present powerless to prevent. Further, museums are recognised by the Educational and Science and Art Departments in that attendance at a museum by school children and science students is, under certain conditions, allowed to count as class attendance, and specimens are loaned to them from the Art collection of South Kensington.

It has followed from the operation of causes such as those indicated that a type of museum has arisen throughout the country which justifies the charge of ill-conceived, lacking in proportion, wanting in utility and inadequately supported and staffed. If we bear the present conditions in mind, and consider the proposed remedies, it is manifest at once that the utmost difficulty would be experienced in the attempt to create a federal staff of scientific experts on the lines which have been advocated by Professor Petrie. The museums are so unequal, their sections vary so strangely, both in ratio to one another and in their value, that the attempt to send to a series of museums a number of specialists, devoting equal amounts of time to each, would result in confusion and failure.

The first plan suggested in the editorial comment of *Natural Science* of August last is practically that of Professor Petrie, with the added proviso that the specialists should be resident, one at each museum, whilst the same round of visits was maintained. Other considerations apart, municipal jealousies would effectually kill the scheme.

The second plan is delightful by reason of its naïve character rather than its practicability. The visits of specialists from the Government museums are red-letter days to the provincial curator, valuable alike for the rare good fellowship meted out to the humbler brother in science, for the gratuitous work done, and the remarkable stimulus and enthusiasm imparted; but that the arrangement and scientific work of provincial museums should be left to the staff of Government museums, increased in numbers for the purpose, is a plan hardly likely to commend itself to the provincial curator or his governors, and even less likely to Parliament, which would need to be approached to decide the question.

There is much in each plan that is admirable, but after all they are in the nature of makeshifts rather than a solution of the question. What seems to be needed is a thorough grapple with the whole question of museum development, and (to borrow a

sentence from *Natural Science*) of the means of "co-ordination, investigation, and effective utilisation of all our obscured scientific and artistic material," together with the creation of means by which those aspiring to take up curatorial work may receive a thorough training.

A somewhat rough-and-ready classification of provincial museums seems to indicate naturally a string of suggestions which might possibly be elaborated into a workable and effective scheme. At present provincial museums may, we think, be placed in three or four classes as follows:—

I. Museums of University Colleges, and of large cities possessing large collections and a trained staff.

II. Museums of important manufacturing centres, or county towns, with usually fair collections and a self-educated staff.

III. Museums of small towns, and of scientific societies which are managed by honorary curators, or once arranged and afterwards left unaltered in the hands of a care-taker.

IV. Museums under the Libraries and Museums Act which are managed by the librarian in addition to his own work, outside help being obtained from time to time. Some of these museums rank in importance with those of Class II.

That a relative importance of provincial museums exists such as that implied in this classification will, we think, be admitted, and upon this assumption the following suggestions are formulated as a means of bringing about that co-ordination and improvement of museum development which is so much desired.

Suggested Plan

I. Creation of an annual museums' grant by Government in aid of provincial museums of the first class, the sum allotted to each being determined by considerations similar to those which guide the application of the present University Colleges' Grant.

Upon the strength of such a grant the Government could charge each museum with a definite scope of work and the attainment and retention of a certain standard of excellence.

For example, a museum might be called upon as a condition of receiving the grant to have special aims such as the following:—

(*a*) The specific task of investigating and demonstrating by collections the natural history, &c., of a defined geographical area of which the museum would be the centre.

(*b*) The formation and continued progress of a good general collection suited to the needs of the area served by the museum.

(*c*) The attainment of a certain standard of excellence and completeness.

(*d*) An adequate staff.

Means could be devised by which certain of the museums in classes II. and IV. could be raised to the standard of class I., so that, speaking generally, the country would become parcelled out into areas, in each of which a museum was maintained, in constant good order, and ministering in a special as well as a general manner to the needs of the population.

II. Remaining museums might be subsidised through the agency of County Councils, upon certificates of efficiency and progress received annually from an accredited visitor, who might be an official of the Government or of one of the first-class museums.

Great help would be rendered to the curators of small museums if, as a condition of the subsidy, a member of the staff of the first-class museum had a seat and vote upon the committee. Such a post might well be honorary, and would be analogous to that which obtains in the case of certain Grammar Schools, a governor serving upon the board by appointment from one of the larger Colleges or Universities.

III. The small museums might be grouped around the larger according to locality and function, and by means of a nominal contribution to the funds of the larger have a claim for help and direction.

The advantages, if a scheme of this sort be applied to the whole country, might be dealt with at considerable length, but it will be less likely to confuse if we narrate briefly what we consider to be the chief.

Museums' aims, management, and development would become organised throughout the country, and lead at the outset to the abolition of a needless overlapping where several museums exist in a large town or city under different management.

More museums of the first class would be created, and the curators of county museums, and students generally, would find help nearer at hand than at present. The conditions of existence of the large museums would require that the systematic study of the fauna, flora, &c., of large districts be prosecuted with diligence and the salient features of each being demonstrated in the museum collections. The whole would necessarily become of prime importance to the public, a permanent memorial of the scientific value of the area served, and of great usefulness to the specialist and the nation.

The small museums might have a similar charge, though, of course, in a smaller and more localised degree, whilst being also required to maintain a good type-collection suited to the wants of the population.

Museums would be brought into direct communication with each other, and the transference of specimens to centres where they would be of most use would be immensely facilitated, while the small museums would naturally become agents of supply of material obtainable in their districts. The whereabouts of material for the specialist would be better known, and be more readily available.

The linking up and increased usefulness of provincial museums would provide a healthy stimulus to local scientific societies, would result in increased and more thorough field work, and do much to aid that federation of remote scientific workers which is so desirable. Moreover, it must not be forgotten that such a chain of museums offers the best means whereby the collections of the humblest and most distant worker might be conveyed to the one best fitted to deal with them.

The plan we advocate is crude, but designedly so, for elaboration provokes criticism of detail rather than of principles, and it is the latter which are all-important to determine in connection with the present phase of the provincial museum question.

HERBERT BOLTON.

III

Cell or Corpuscle?

ONE of the youngest and most vigorous among the sciences is that which has been named Cytology.

Its strength, in all probability, is due to the fact that it has sprung from a broad foundation, and that it still rests, not upon one, but many pillars of support. The botanist, the zoologist, the physiologist, divergent from one another as their daily walks unfortunately are, yet agree to join hands over the common basis of their sciences —the organic cell.

With such splendid results already gathered into its encompass and with the hope and promise of such a brilliant future before it, it is all the more to be regretted that the language of Cytology is not only in contradiction to common sense, but such that it must assuredly lead to endless perplexities.

The word cell, the very watchword of the science, is one that in this mouth means one thing, in that quite another.

Dealing with the lower plants, the botanist will speak of the 'swarmspores' as cells, whilst when he turns to the phanerogams he will apply the same term to the elements of cork or sclerenchyma, notwithstanding the fact that the former consist of protoplasm and nucleus without a cell wall, whilst the latter are composed of cell walls without either protoplasm or nucleus.

Such inexactitude as this, such a want of definite expression, must hang as a burden around the neck of the science, impeding its progress at every turn.

More especially is it from the side of the student of vegetable life that this confusion is felt, the zoologist, although he uses the word cell in a sense which stands in flat contradiction to its every-day meaning, yet attaches a significance to it which is clear and precise in his own mind.

However desirable, therefore, it may be for a more common sense terminology to be introduced into zoology as well as botany, it is not absolutely essential for scientific advance. When it is remembered also that any change of nomenclature in this respect must involve with difficulties the immense mass of zoological literature which has gathered together since 1839, it should make us pause before we suggest giving up the word cell, in the zoological sense, in favour of energid or biophor.

It seems to me that there are only two possible ways of reform open to us, and both are accompanied with grave difficulties.

We may introduce that change from the word cell to that of energid (Sachs) or biophor (Hansen), which, it has already been pointed out, will bring countless troubles with it in respect to the old and classical literature; or we may effect an alteration in nomenclature within the science of botany itself. Retaining the word cell to express protoplasm and nucleus, we may bring in a new name for the cell wall minus its living contents. Here also great confusion would result to the older writings, but the confusion would be within a more limited sphere. As we have already mentioned, it is in the province of the vegetable kingdom that the difficulties with regard to the meaning of the term 'cell' have arisen, and in reading the older memoirs we are frequently brought to a pause to enquire whether it is cell-wall or living cell-contents that are being referred to under the name of cell. The perplexities accompanying a substitute for the word cell in its significance of cell-membrane would, therefore, not be so heavily felt as those associated with a new word for the protoplasmic contents. The changes involved would be heavy within their sphere (it is not simply the word cell but the compound expressions which have been formed from it which would have to give place to the new order of things), but still this range would be a fairly limited one. It would be only the single science of botany, already perplexed with difficulties of meaning and not the additional provinces of zoology and animal (and human) physiology, which are clear in their use of the word, which would suffer from the innovation. What substitute might be employed for the word cell in its application to the membrane and the cavity included by it, is not easy to see—perhaps the word 'vesicula' is not altogether inappropriate, but if changes on these lines should ever prove applicable, it will then be time enough to look about for a new term.

The first alternative, which was mentioned above, is the one which hitherto has alone been dealt with by biologists.

Sachs, in his two notes in Flora (1892, 1895), has proposed to call the nucleus, together with the protoplasm governed by it at any time, an energid. If such an energid be included within a membrane it is to be spoken of as a cell. The distinguishing characteristic of an energid is the living element (protoplasm and nucleus), whilst that of a cell is the membrane.

From this point of view, therefore, the swarmspore of *Ulothrix* is an energid, whilst immediately that it forms a wall around itself it reaches the further dignity of a cell: the elements of cork tissue are also cells. In plants of the type of *Vaucheria* the protoplasm is studded with nuclei, and the whole mass is enclosed within a single cell-membrane. Each nucleus may be conceived as exerting its in-

fluence over a limited area of protoplasm, and ruler and ruled may together be mentally mapped off as a unit. The body of *Vaucheria* can therefore be described as consisting of a number of units—the energids—enclosed within a single cell wall.

Quite lately another small work dealing with these matters has been written by Dr Adolph Hansen, Professor of Botany at the University of Giessen.[1]

That part of the little book which deals with the 'Geschichte' is most carefully and judiciously written, and it at the same time has a life and vigour in its sentences that fire our enthusiasm for the subject. When we turn to the latter pages of the pamphlet in which the 'Kritik' is embodied, it awakens very mixed feelings within us. It sets us thinking, which is a good thing, but nowhere does it bring conviction with it. Where the views are most definitely stated we feel the greatest doubt, and where the arguments should be the most irresistible we are the least convinced.

Glancing at what is written on pp. 50-58, we see that the zoological definition, if I may so term it, of a cell is taken as the starting-point of the argument. The zoologist (*e.g.* Professor Oscar Hertwig) defines a cell as a little mass of protoplasm that includes a nucleus within its substance.

From this point of view Hansen maintains that the general assertion 'that plants are composed of cells' is untenable, since the larger mass of them is built up of cell-walls; moreover, whilst a naked swarmspore may be correctly named a cell, it ceases to be such immediately that it forms a wall around its surface.

In considering these statements we must first ask whether it is really a general assertion that 'plants are composed of cells.' I do not think so. I myself, and all whom I have ever heard, have always, both in animal and plant histology, stated that the plant or the animal, as the case may be, consists of cells and the products of cells. Our author meets this qualification in part, perhaps, by saying that those who assert that plants consist of cells (in the zoological sense) approach the difficulties in the above cases by regarding the membrane as 'secondary' or 'unessential,' and in that case he goes on to argue how are we to look upon *Caulerpa*, whose whole form and existence is determined by the cell-wall?

Secondary and unessential the cell-wall certainly is, however, when we compare it with the protoplasm and nucleus.

The latter determine whether a structure is living or dead; the former merely influences the manner of life.

Caulerpa as a genus is undoubtedly dependent on the presence of a membrane, but *Caulerpa* as a living thing is due to the co-

[1] "Zur Geschichte und Kritik des Zellenbegriffes in der Botanik," von Dr Adolph Hansen. Giessen: J. Ricker, 1897.

existence of protoplasm and nuclei. Hansen cannot maintain that the fibres and inorganic parts of a bone are not 'secondary' in importance when placed side by side with bone corpuscles, and yet the whole form and existence, nay, the whole use of the bone, depends upon their presence.

Another, and perhaps even more analogous case, is furnished by the elastic fibres of certain ligaments (*Ligamentum nuchae*): the whole existence of these bands is justified by the elastic fibres they contain, and yet when valued against the protoplasmic corpuscles upon which their origin depends, these fibres must be relegated to a 'secondary' place.

The white connective tissue fibres, the elastic fibres, and the vegetable membranes are all of enormous importance in the phylogenetic development of the organism; without them neither animals nor plants would ever have been able to rise above the state of mere flabby masses of protoplasm of limited size, but that which stands higher than race development, the existence of life is interwoven with the protoplasm and its nucleus. It is only compared with this high standard that we dare speak of the membrane of plants as 'unessential' or 'secondary.' When Hansen, however, a few lines further on, indicates that those who use these two words when speaking of the cell-wall do so alternatively with the expression of 'no importance,' he altogether misunderstands their position.

The next few pages are occupied with a criticism, more or less destructive, of Sachs' views on energids; this is followed by a proposed improvement. The word energid shall be dropped; in such cases as the *Siphoneae* the separation of energids is artificial, and therefore to be avoided; in its place the whole contents of a single membrane, or the whole mass of a membraneless organism, shall be named a 'biophor,' whether this be uni- or multi-nuclear. When a 'biophor' is enclosed by a wall it becomes a cell. "The cell," he adds, "consists always of a biophor and a membrane." When, therefore, a few lines further, he says that with the adoption of this nomenclature there is no reason why the elements of wood or cork should not be named cells—cells that have lost their biophors—certain inconsistencies of statement become apparent. Dr Hansen himself points out that the name biophor has already been used by Weismann in quite another sense. I cannot think that it would be wise therefore to employ it in this new relation, as even supposing Weissmann's biophors do not prove all that was hoped of them, they certainly will take a permanent historical value, and fresh troubles will appear on the horizon in consequence.

Although the last eight pages of Hansen's pamphlet seem to me to be open to criticism, the fifty foregoing pages can only give pleasure and satisfaction to those who read them.

The historical account of the cell theory is one of the best that has been written for a very long time, and the frequent apt quotations from and references to the original memoirs breathe a spirit of life into the story which one only seldom finds in these retrospective writings.

Before leaving the subject of reform by change in the name of 'protoplasm plus nucleus' it may be mentioned that if a new term is to be given to these parts, a more justifiable one than energid is that of 'corpuscle.' It is a name which is already used in this sense by the zoologist: he speaks of the white blood corpuscle, the bone corpuscle, etc., and its introduction into botany would be far from insurmountable. To speak of a swarmspore as a 'corpuscle' would be both common sense and simple. The membrane which this 'corpuscle' manufactures might be termed a 'cell,' and we should be speaking both logically and intelligibly when we spoke of the 'corpuscle' which lies within the 'cell' when we dealt with an element of the cambium, or if we spoke of an aggregate of corpuscles lying within the cell when we treated of *Vaucheria*. When the elements of cork came into view they would be cells pure and simple.

If a collective name for the living contents of a cell be required we might resort to the terminology which has already been employed by Professor Strasburger. A corpuscle, in the above sense, might be described as consisting of cytoplasm and nucleus, and the name protoplasm be applied to the cytoplasm or the nucleus, or both together, whether one nucleus or many, or none, were associated with the cytoplasm.

In what has been written above I only wish to throw out a few rough suggestions which may perhaps help the cytologist in a small degree as he gropes in the darkness for the right path.

It is either the wall of the cell or the living contents of the cell which must be re-named. If it be the former the difficulty will fall upon the shoulders of the botanist but leave the zoologist unharmed; if the latter, much depends upon the nature of the alteration.

If we radically change things by bringing into use a new name (energid, biophor) endless perplexities will undoubtedly arise, but if we resort to a word like that of 'corpuscle,' and employ it in the way indicated, the troubles may be smoothed over. It is a word that the animal histologist has already often used, and one that is not really difficult for the botanist to adopt; it is one that is common sense, and which likewise would allow the term cell to be brought into the same category.

I will leave matters here, however, for others to judge and to criticise.

RUDOLF BEER.

IV

Fossil Apodidae

EVERY group of animals contains in itself its own record if we could but decipher it. The zoology of the future is bound under the fascination of this idea to devote itself to an ever closer comparative study for the express purpose of gaining insight into the lines of their past development. We have no royal roads, and the hopes which were held out to us a few years back that embryology would provide us with one have not been realised. It can only supply us with hints, the full meaning of which we must learn by other methods. Most hopeful of good result are those groups in which allied forms, recent and extinct, offer themselves in sufficient numbers for comparison. None can compare in this respect with the crustacea. There is an enormous wealth of known crustacean forms extending upwards from the very lowest fossiliferous strata and still swarming in every suitable part of the globe. The problem has long been to find a genealogical key to reduce this immense stream of organised life to some order of development.

The first and most striking feature noted was the fact that all but the most extreme forms are segmented, and the natural inference was that the common ancestor of the whole immense family must have been some less specialised segmented form. Of recent years the attention of zoologists in search of the most primitive form among existing crustacea has been concentrated upon the freshwater *Apus* which appears sporadically in rain pools all over the world. When the pools dry up the eggs remain in the mud; indeed it is said to be a necessary condition of development that they should be so dried. How long they remain capable of development is not known. These facts are interesting because they suggest to us a way in which *Apus* may have survived, practically isolated from the struggle for existence, almost unchanged from the days when the crustacea first appeared on the planet. Those who claimed the primitive character of *Apus* were not disconcerted by the absence of fossil remains which could be definitely assigned to *Apus*. There was, of course, always the hope that such might be found, and further there was the striking fact to which they could appeal that one of the most prolific of early crustacean forms, the Trilobites, possessed many characters in common with *Apus*. The great Paleozoic family of the Trilobites

are thus claimed as relations of the modern *Apus*. Now, however, that the claims of *Apus* to have been co-existent with and in some way closely related to the Trilobites are being reasserted on the basis of a new interpretation of the morphology of the former and of new facts as to the organisation of the latter—other mysterious relations are cropping up. With two of these—viz., *Protocaris* from the Lower Cambrian, and *Dipeltis* from the Lower Carboniferous, Mr Schuchert deals in the *Proceedings of the U.S. National Museum* (vol. xix.). Neither are quite new, the former having been figured and described by Walcott,[1] and the latter, from very imperfect material, by Packard. Mr Schuchert gives a new figure of *Protocaris* showing a little more detail, refigures the type specimens of *Dipeltis*, and is fortunate enough to be able to describe three new and almost perfect specimens of the same, one of which represents a new species.

Mr Schuchert has no hesitation in claiming both these as Apodidae, a welcome claim to any who, as above described, have interested themselves in placing *Apus* with the Trilobites at the root of the crustacean phylum. But inasmuch as Mr Schuchert only deals with these new claimants very generally, it has been suggested that a cross-examination of them from this special point of view would not be uninteresting.

Protocaris marshi

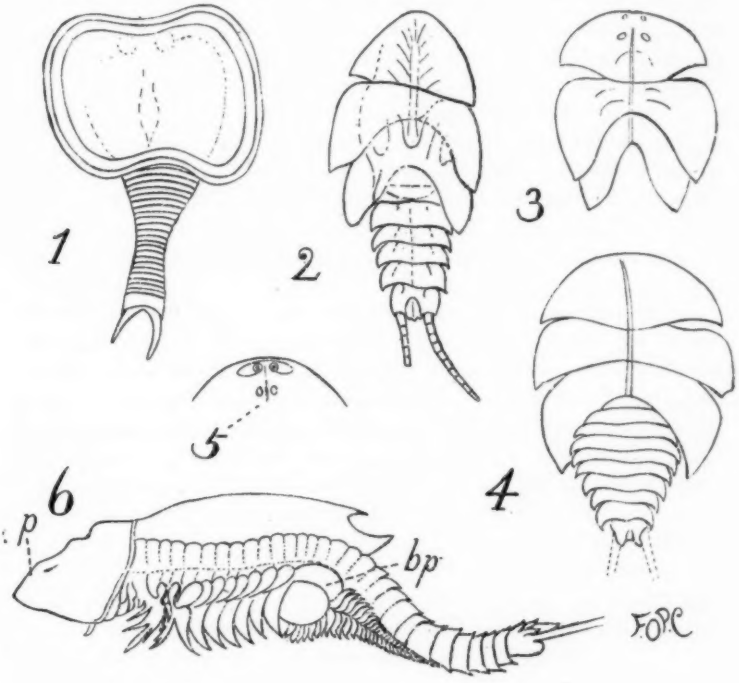
This fossil, only known in one specimen from the very oldest fossiliferous strata, speaks for itself (see fig. 1). Its large cephalic shield spreading backwards over the trunk segments, the extraordinary shortness of these segments in strong contrast with the wide anal segment with its pair of cercopoda, and lastly, which is a new detail added by Mr Schuchert, the pair of circular markings suggesting eyes near the anterior margin of the carapace—all proclaim its close affinity to *Apus*. Indeed, on first acquaintance with this fossil, I went so far as to suggest that it might with advantage be called *Apus marshi*.

Since studying Mr Schuchert's paper, however, I have been struck by two features which seem to me to have been generally overlooked, one of which is of prime importance. A comparison of the abdominal segmentation of *Protocaris* and *Apus* appears to show that the former retained a primitive condition which has been secondarily lost in the latter.

One of the principal arguments in favour of the great antiquity of *Apus* was found in the fact that a great and varying number of posterior segments are fixed in a rudimentary condition. The evidence for this was found in the progressively diminishing sizes of the

[1] *Bull U.S. Geol. Survey*, 10, 1884, p. 50, pl. x. fig. 1; *Amer. Nat.*, 1885, p. 293; *Mem. Nat. Acad. Sci.*, iii. pt. 2, p. 145, pl. v.

limbs (see fig. 6), each pair being provided with a pair of abdominal ganglia. It was, of course, more or less of an assumption that these limbs and ganglia represented true segments, because to all appearance the true segments are marked off on the body as shown in the fig. 6. Further, authors were not wanting who definitely claimed that the apparent segmentation was the true one, and that the multiplication of the limbs, as many as four, five, and even six to one single segment, was due to some kind of secondary reduplication



1. *Protocaris marshi*, Walcott; 2. *Dipeltis carri*, Schuchert; 3. *Dipeltis diplodiscus*, Packard; 4-5. Another specimen of the same, showing the under surface and part of the upper with the eyes; 6. *Apus glacialis*, var. *spitzbergensis* from the side, with left half of the carapace cut away to show the whole trunk; *bp.* left limbs of the eleventh segment modified into a brood-pouch, *p.* pore leading into the water sacs over the eyes. [Figs. 1-5 after Schuchert.]

of organs on one and the same segment. The present writer, on the contrary, maintained that, as at the front end of the body each pair of limbs with its pair of ganglia corresponded with a true body segment, the whole series of the limbs should be taken as true segmental structures, and where, after the eleventh segment, marked by its brood pouch (*bp.*), the limbs begin to diminish in size, there being more than one to each external division of the body, these

external divisions must be considered as secondary rings and not as true segments.

The correctness of this suggestion seems to me to be entirely borne out by the abdominal segmentation of *Protocaris* in which this secondary ringing has not taken place. If a body-ring were marked round the abdomen of *Apus* for each of the pairs of limbs, leaving out the most minute at the posterior end (*cf.* figs. 1 and 6), we should get a condition, at least for the limb-bearing portion of the abdomen, not unlike that shown in *Protocaris*, in which a multitude of very small segments (though not diminishing quite so clearly as they would in *Apus*) are in striking contrast with the anal segment.

It should, however, be noted by the way that this interpretation of the segmentation of *Apus* which receives such unexpected and welcome support from *Protocaris* was greatly complicated by the presence in *Apus* of a varying number of limbless segments in front of the anal segment. These still require explanation; at present I am inclined to look upon them as secondary reduplications of the anal segment.

This difficulty must not, however, be thought to stand in the way of my interpretation of the segmentation of *Apus*. That interpretation has already received abundance of support from the fact that the same fixation of rudimentary segments is found in the Trilobites. Inasmuch, however, as the relationship between *Apus* and the Trilobites is still a matter of discussion, it is especially welcome to obtain direct evidence from a fossil whose close affinity with *Apus* cannot be for a moment doubted.

The second point arises from the peculiar shape of the shield. Mr Schuchert describes it as subquadrangular, and quotes Clarke's suggestion that it has probably been subjected to some horizontal distortion in the shale. The longer I contemplate the figure of this shield the more convinced I am that it has simply been flattened out, and that in its original shape it was folded down at the sides of the body. Not only do the two anterior lateral projections of the shield suggest this, but the absence of the usual spikes at the postero-lateral corners of the shield are quite in accord. In *Apus* these spikes are turned up somewhat on to the back (fig. 6), and in the Trilobites they are spread out wide of the body in the horizontal plane. If the shield were folded down at the sides, these spikes would be a serious danger to the limbs and abdomen, and would be sooner or later dispensed with. Whether, again, the dotted lines running along the shield, shown in Mr Schuchert's figure (reproduced in fig. 1), lend any support to this view, I should not like to say, because we have no means of getting at their true meaning, but they certainly suggest to my mind a dorso-ventral flattening.

We have then, it seems to me, a simple and natural interpretation of the shape of the shield of *Protocaris* without assuming any other distortion than that due to flattening. Had there been any horizontal distortion, it would, one would expect, be more apparent in the abdomen than in the shield. I am therefore disposed to look upon *Protocaris* as an *Apus* in process of folding down laterally its whole carapace, a modification which, as I have shown in some detail elsewhere,[1] would lead on to the peculiar organisation of the ostracods. I lay stress upon the word 'whole,' because if only the free lateral flaps behind the head region are folded down, we should get a form which might lead on to the other bivalve-entomostraca, the Daphnidae and Estheridae. In making these suggestions I am again taking up my original position that *Apus* is the protonauplius of authors, and that from it or its young stages all the crustacea can be deduced. I may add indeed that nothing which has been said during the last five years has shaken me in that conviction, based originally upon my study of *Apus*. On the contrary, all the new facts which have come to light have tended without exception to confirm it. I refer mainly to the brilliant researches of Beecher on the limbs of the Trilobite, *Triarthrus*, and to these fossil Apodidae now under discussion.

The whole question, however, must of course be decided solely by the evidence; hence, I may remark in passing, it is somewhat surprising to find a zoologist declaring that he has no "sympathy (*sic*) with the peculiar phylogenetic speculations of Bernard." Antipathy against the views of a fellow-worker, however unscientific such an attitude of mind may be, is perhaps excusable, but it is not so excusable merely to refer readers to a semi-popular summary and not to the papers containing the detailed evidence for the hypothesis condemned. Readers could then judge for themselves whether these 'peculiar phylogenetic speculations' are speculations at all, and not rather necessary deductions from established facts.

Mr Schuchert's claim that 'eyes' can be faintly seen on the specimen will be noticed below.

Dipeltis

The claims of this fossil, of which only four specimens are known, to belong to the Apodidae, seem to me far more intricate than are those of *Protocaris*. It appears at first sight as if it might be a transition form between *Apus* and the Trilobites, and yet it only (so far as at present known) appears on the scene in the Lower Carboniferous, when the Trilobites were already beginning to pass away.

[1] "Apodidæ," Section xv., p. 252.

We note, particularly at the outset, that Mr Schuchert evidently assumes that the head of *Dipeltis* corresponds to the head of *Apus* with its five segments. There is no reason to doubt the correctness of this assumption.

When analysing the differences between *Apus* and the Trilobites,[1] I came to the conclusion that the essential distinction between them was as follows:—In *Apus*, after the formation and fixation of the head region out of five fused segments, the cephalic shield grew backwards as a large free carapace over the trunk segments, which remained cylindrical, whereas in the Trilobites the head shield did not grow backwards freely over the body, but was repeated, by the familiar process of segmental repetition, on each of the trunk segments, giving rise on them to their pleural extensions. This curious difference in the mechanism of development resulted in two such apparently distinct forms as *Apus* and the Trilobites.

Dipeltis now comes on the scene. Its general appearance is that of *Apus*, but its repetition of the head shield is that of the Trilobite. The head shield and its two large segmental repetitions together appear as if they imitated the true free carapace of *Apus*. They only appear to cover the trunk segments, as the carapace of *Apus* covers its cylindrical trunk. Further, the trunk segments also show in their small pleurae a slight repetition of the head shield, or rather of the last of the two large repetitions of the head shield. These are unmistakable Trilobitic characters. On the other hand, the anal segment with its pair of cercopoda, the arrangement of the large eyes with anterior eyes or pores at the front of the head, the smooth round forehead without glabella or furrows, are unmistakably apodidan features. The animal looks remarkably like a cross between an *Apus* and a Trilobite! The only explanation I can suggest of this singular creature is that it resulted from a second attempt on the part of the main- or *Apus*-stem of the crustacean phylum to adopt the Trilobitic modification; that is, for a second time these primitive crustacea tried the segmental repetition of the early head shield instead of its free backward extension as a carapace. I would explain the *Apus*-like appearance of this second attempt as being due to the fact that it started from a true *Apus*, whereas the Trilobites owe their peculiar characters to the fact that they branched off before the *Apus* type was fixed—*i.e.* while the head region was still in process of formation—hence the glabella, the transverse furrows, and the uncertain position of the eyes on the head shield, with the three longitudinal regions running along the dorsal surface due to the segmental repetition of that head shield.

Turning, in conclusion, to the eyes in figure 3, these are shown very like those of *Apus* as to position and arrangement, while in front

[1] *Q. Journ. Geol. Soc.*, vols. li. and lii.

of these larger eyes are two spots. These are also found on another specimen, part of which is shown in figure 5, copied from Dr Schuchert's paper. In *Apus* in front of the paired eyes is a median pore (fig. 6, *p.*) leading into the water sac, in which these paired eyes are now sunk. In the base of the channel leading from this pore to the water spaces above the eyes a body called the median eye is suspended. The structure of this eye and its probable origin —as suggested by its structure—out of a pair of anterior eyes I have discussed elsewhere.[1] It is therefore of some interest to me to find an animal, with such claims to be a relative of *Apus*, having a pair of what appear to be eyes in front of the usual pair. It is true that this anterior pair persists in *Limulus*, but here again the relationship between *Apus* and *Limulus* is still matter of discussion. It is therefore once more pleasant to find what appears to be a direct confirmation of one's morphological deductions in an animal certainly related to *Apus*. What the peculiar ovals round these anterior 'eyes' are in fig. 5 it is impossible to say. It may be that here we see these eyes being drawn into the median line and below the surface.

In *Protocaris* I should regard the two eyes, suggested in figure 1, as corresponding with the paired eyes of *Apus:* perhaps owing to the bending of the carapace they are sunk deeper in the water sacs.

I quite agree then with Dr Schuchert in calling these early crustaceans fossil Apodidae. *Protocaris* I suggest is a modification of *Apus* in the direction of the bivalve Ostracods, *Dipeltis* as a second attempt of a true *Apus* to adopt the Trilobitic modification of repeating the head shield as pleurae along the trunk segments, while preserving for some reason or other the habit of *Apus*.

HENRY M. BERNARD.

[1] "Apodidae," pp. 100-111.

V

Reproductive Divergence: A Rejoinder

IN the last month's number of *Natural Science* (p. 317) Dr Karl Jordan criticises a theory which I had briefly suggested in a previous number of the journal (p. 181), and which I had entitled "Reproductive Divergence: an Additional Factor in Evolution." I had there maintained that my theory differed essentially from Romanes' theory of Physiological Selection, for I endeavoured to show that if the less similar individuals in any species were at the same time less fertile *inter se* than the more similar, it would necessarily follow that in the course of succeeding generations these members would diverge more and more from each other, till eventually two or more new and mutually sterile species would be formed. I still hold, in spite of Dr Jordan's view to the contrary, that this theory is essentially different from Romanes', which maintains that if a portion of the members of a species happen to be sterile with all the other members, they will, in virtue of this physiological barrier, be enabled to vary independently of the parent stock, and so give rise to a new species.

I stated that my theory was made up of two parts, one of which was capable of mathematical demonstration, whilst the other could only be verified by experiment. Dr Jordan takes exception to the former, but accepts the latter, he holding that the "correlation between morphological characters and fertility of the specimens of a species cannot be denied." He also adduces an additional instance in support of the existence of this correlation.

The mathematical demonstration of the validity of the theory which I gave was, it would seem, rather too brief for its purpose. In excuse I must plead that my paper was intentionally of only a short and preliminary nature, as I thought a more extended discussion had better be deferred till I had more experimental evidence at my command. As, however, Dr Jordan appears to have entirely misunderstood my reasoning, he holding indeed that in the particular example I adduced to prove a divergence of character there would on the contrary be a convergence, I must now endeavour to explain the mathematical basis of the theory more fully. Thus of its validity I am convinced there can be no question. Its adequate demonstration depends only on skill in manipulation of figures, though this I am afraid I do not possess.

The fresh example I propose to adduce is founded on actual data, namely, the measurements of human stature in the case of parents and their offspring, which are given by Galton in his work on "Natural Inheritance" (p. 208). Here the relations between the stature of 205 parents of each sex and of their adult offspring are classified. The mid-parents I split up into three nearly equal groups—viz., those below 67·8 inches in stature, those from 67·8 to 69·2 inches, and those above 69·2 inches. These groups were found to give rise to more or less equal numbers of children. The numbers of children of each stature which were given birth to by each of these three groups of short, medium and tall mid-parents were then determined, partly by plotting out the various numbers and calculating from the smoothed curves, and partly by direct enumeration of the numbers. By then taking means between the numbers of tall children produced by tall parents with those of the short children produced by short ones, and also between the numbers of short children produced by tall parents with those of tall ones produced by short parents, roughly speaking the following mean values were arrived at :—

	Short.	Medium.	Tall children.
100 short parents give .	54	31	15
100 medium . . .	31	38	31
100 tall . . .	15	31	54
	100	100	100

From these figures it is seen that the percentage numbers of children are given, and not the actual numbers. This plan was adopted in order that the numbers of children might be kept the same as that of the parents. We see, therefore, that if 100 short, medium and tall parents of each sex be taken, the numbers of short, medium and tall children will still remain at 100, in spite of the fact that the children of each group produced by the different parents vary from 15 per cent. to 54 per cent.

In addition to the children produced by short, medium and tall parents, it is for our purpose necessary to know the numbers produced by intermarriages of short and medium, and of medium and tall parents. These numbers may be approximately obtained by taking means between the percentages for short and medium parents on the one hand and for medium and tall ones on the other. Thus :

	Short.	Medium.	Tall children.
100 short and medium parents give	42·5	34·5	23
100 medium and tall . . .	23	34·5	42·5

It is also necessary to assume, as Galton has shown his statistics warrant us in doing, that short and tall and also tall and short

parents give rise to the same numbers of children in each group as do medium parents.

If we now take the case of 900 parents of either sex and divide them into three groups of 300 short, medium and tall individuals, it will be found that, as the result of their chance intermarrying, they will give rise to the following numbers of offspring of each sex:

293 short. 314 medium. 293 tall.

These numbers thus differ slightly from the 300 of each group which were started with, but it is not possible to arrange the numbers resulting from the various intermarryings so as to give quite identical figures. This, in fact, is not necessary, as it is only sought to compare two different cases, in one of which a correlation between fertility and stature is absent, and in the other of which it is present. This latter case we will now proceed to discuss.

Let it be granted that, in pursuance of this principle, 100 parents intermarrying with parents of similar stature now give rise to 120, and not 100 offspring of each sex. When on the other hand they marry with moderately taller or shorter individuals, let the number of offspring remain unchanged, and when tall or short parents marry with short or tall, let only 80 offspring instead of 100 arise. If now the same 900 parents of either sex intermarry according to the laws of chance, it will be found, on calculating out the numbers in accordance with the data given above, that the following numbers of offspring will be produced:

Short.	Medium.	Tall.
300·6	318·8	300·6
instead of 293	314	293.

That is to say, the numbers of short and tall individuals will have increased by 2·59 per cent., but those of the intermediate ones by only 1·52 per cent. There is thus a gain of about 1·07 per cent. in favour of the extreme individuals over the intermediate ones, or supposing the original 300 medium individuals had again been produced, there would now be 303·2 instead of 300 tall and short ones. This seems a very small amount in comparison with the fairly large differences of fertility we assumed to be present. It is, however, none the less genuine, in spite of its smallness, and it will of course become increasingly larger in succeeding generations. Thus supposing that in the next generation respectively 303·2, 300, and 303·2 short, medium and tall individuals of each sex intermarry, the short and tall offspring will be increased by slightly more than 1·07 per cent.; by in fact about 1·08 per cent. There will now, therefore, be $300 + 3{\cdot}21 + 3{\cdot}24 = 306{\cdot}45$ short and tall offspring, instead of 300. In succeeding generations the increase will gradually become more and more marked, and the divergence

of the race into two distinct races more and more obvious. Again, I had supposed that the more widely any two individuals differed, the less on an average would be their mutual fertility. Hence this principle of reproductive divergence is a cumulative one, and the later stages of divergence will be very much more rapid than the initial ones.

Again, Dr Jordan objects to my statement that as the race diverges, shorter and longer individuals than those originally present will gradually be evolved. This seems to me to be so obvious as to scarcely need demonstration. Thus, let us suppose for the moment that our 900 individuals of each sex are split up into 450 short individuals and 450 tall ones. The members of each of these groups will deviate in either direction from the average size in the same proportion as the members of the original single group did. For instance, in the 928 adult offspring obtained in the above-mentioned anthropometric data, the mid-stature or median of the whole group was about 68·2 inches, and 10 per cent. of the group were below 64·5 inches in height. Supposing now the median of the new group of short individuals be 65·8 inches, it follows that 10 per cent. of this group will be below 62·2 inches in height. That is to say, individuals shorter than any of those originally present will have arisen. The tall individuals will, of course, deviate in a similar manner in the opposite direction.

This fresh attempt to demonstrate the correctness of the principle of reproductive divergence will not, I am afraid, appear much more easy to understand than the former one, but it at least has the merit of being roughly founded on actual data, so that fewer preliminary assumptions are necessary, and the result obtained is a more absolute one, and is moreover independent of the Law of Regression towards mediocrity.[1] To me it seems the principle is sufficiently obvious without the help of any mathematics at all, if it be looked at in the following manner. Let any number of individuals in a species be divided up into two groups—the larger ones and the smaller. Then if there be a correlation between size and fertility, it follows that those larger individuals which happen to breed with the smaller ones will give rise to fewer individuals of intermediate size than they would have done if there had been no such correlation. That is to say, the race will begin to diverge, and as this divergence is cumulative, it will ultimately split up into two or more new races.

H. M. VERNON.

[1] "Natural Inheritance," p. 95.

VI

Professor Schiller on Darwinism and Design

I SUPPOSE that it must, on the whole, be reckoned as an encouraging sign of gradual advancement that, from time to time, articles dealing with physical science are allowed to appear in the monthly *Reviews;* for the appearance of such articles indicates that the editors of these strictly commercially-conducted *Reviews* can safely reckon upon a tolerable percentage of their readers being interested in physical science.

This apparent gradual spread of an interest in physical science is comforting; and it is good that the huge section of the public, who never by any chance read scientific books or scientific periodicals, should yet, through the medium of the *Reviews*, acquire some slight taste for physical science and some trifling knowledge of recent advances therein. From this standpoint, then, the practice of inserting scientific articles in the lay *Reviews* is much to be commended; but it is not to be denied that there are compensating disadvantages, and these are due to two factors.

In the first place, the editors of these *Reviews*, being usually—like most other "well-educated" Englishmen—utterly ignorant of physical science, are hopelessly unable to estimate for themselves the value of any scientific article submitted to them, and are thus entirely incapacitated from exercising any truly editorial functions with regard to such articles; and, in the second place, all these *Reviews* are conducted upon strictly commercial principles, being regarded primarily as money-making machines, and only secondarily as organs of education and enlightenment. Both these factors conspire to bring about one and the same result, viz., that the editors taboo all articles not signed by a big name—which is at once the guarantee of profit to their purses and of safety to their ignorance—and are naturally tempted to exclude controversial replies. Usually their assiduous worship of big names safeguards them from any fiasco; but now and then there creeps into a *Review* an article which betrays only too signally the fact that it was entirely unintelligible to the "editor," and that that omnipotent functionary was educationally disqualified from perceiving the extraordinary nature of the statements and arguments appearing in his *Review*.

To this category seems to belong the article on *Darwinism and Design*, contributed by Professor F. C. S. Schiller to the *Contemporary*

for June last—an article that, though dealing with physical science, bears the most obvious traces of a metaphysician's handiwork. I certainly do not propose to criticise this article in detail; for such a criticism, to be at all adequate, might require an article a good deal longer than Professor Schiller's own: and, fortunately, any such detailed criticism were superfluous; firstly, because all readers who have any familiarity with biological science may perceive for themselves the errors into which Professor Schiller has fallen; and, secondly, because those readers of his article who are entirely ignorant of the subject would hardly be among the readers of *Natural Science*, nor would they perhaps be affected by my arguments even if they read them. I propose, therefore, merely to call attention to two or three notable points in *Darwinism and Design*.

The article, which opens with somewhat of a flourish of trumpets, and excites expectations that are by no means realised in the sequel, is directed to prove that 'Darwinism' has not necessarily excluded the possibility of a teleological conception of organic nature; but that, properly scrutinised, evolutionism rather strengthens the argument from 'Design' than otherwise.[1] Now Professor Schiller makes one or two initial omissions of a notable character. He intentionally confines himself to "living nature," thus putting aside altogether the awkward question as to whether the evolution of solar systems be, or be not, ascribable to Design; and he writes as though evolutionism and Darwinism were the same thing—as though to demonstrate an error in any one of Darwin's initial assumptions were to at once clear out of the way all biological objections to the teleological conception of the world. The former omission is highly significant of the philosophical value, or otherwise, of Professor Schiller's article; the latter oversight appears to me to vitiate his entire argument, and to render it little more than a beating of the air; and one is tempted to say that the article would have been topical in 1860, but is a generation out of date now.

Professor Schiller is good enough to tell us that the old-fashioned argument from design was rotten even before the advent of Darwinism; but he proposes to recast the argument in such fashion that 'Darwinism' shall be no obstacle, but rather indeed an assistance, to the teleologist. He tells us that "before the argument from design has any theological value, two things have to be shown: (1) that intelligence, *i.e.*, action directed to a purpose, has been at work; and (2) that the intelligence has not been that of any of the admitted existences."[2] The former part of this statement reads rather curiously; but it only means that, for the author's purpose, it is essential to prove that the adaptations in organic nature have not been brought about solely by a blind mechanical process, but

[1] Cf. p. 144.

[2] *Contemporary Review*, p. 868.

are due, at least partly, to intelligent effort on somebody's part. To make good this position, Professor Schiller has recourse to somewhat strained arguments. For instance, he suggests that "a complete denial of design in nature must deny the efficacy of all intelligence as such. . . . If that view were true, we should have to renounce all efforts to direct our fated and ill-fated course adown the stream of time" (p. 868). But this ingenuous attempt to make the anti-teleologist damn himself—on the plea that logically he is bound to deny the efficacy of all intelligence if he deny the argument from design—seems to me a mere trifling with words, and recalls the old saying that "words are wise men's counters, they do but reckon with them; but they are the money of" metaphysicians. Professor Schiller must know perfectly well that the "denial of design" is merely an elliptical phrase meaning the "denial that the adaptations in nature evidence the design of an over-ruling Divine intelligence"; and that this denial, neither logically nor historically, implies anything so absurd as the denial of "the efficacy of all intelligence"—such intelligence itself being regarded by consistent evolutionists as a notable adaptation to our environment. But this playing with words, this use of words in Humpty-Dumpty's portmanteau fashion, so that two very different meanings are packed into one word, is typical of Professor Schiller's article. Of this we have a glaring example in the following passage, which is, to a certain extent, the key to the author's position:—

"The ease with which the Darwinian argument dispenses with all intelligence as a factor in survival, excites suspicion. It is proving too much to show that adaptation might equally well—*i.e.*, as completely, if not as rapidly—have arisen in automata. For we know that we ourselves are not automata, and strive hard to adapt ourselves. In us at least, therefore, intelligent effort is a source of adaptation, and the same will surely be admitted in the case of the higher animals. . . . Intelligence therefore is a vera causa, as a source of adaptations, at least co-ordinate with natural selection; and this can be denied only if it is declared inefficacious everywhere—if all living beings, ourselves included, are declared to be automata. . . . If, however, intelligence is readmitted as a vera causa, there arises at least a possibility that other intelligence besides that of the known living beings may have been operative in the world's history" (p. 871; spaced type mine).

It is worth while to realise the fallacy involved in this passage; for, when once that is realised, there is not much left in our author's case. The whole fallacy lurks in that word adaptations. It is obvious that individual animals do functionally adapt themselves in many ways to their environment; and that, the greater their intelligence, and the better consequently their adaptations, the greater

their chance of life. But, to Professor Schiller's argument, it is essential to show (1) that structural adaptations can be, and are, thus brought about; and (2) that such adaptations are inherited. This Professor Schiller does not even attempt to prove; but it is the crux of the entire argument. If Professor Schiller can prove that individual intelligence is capable of producing direct structural modifications — *i.e.*, modifications at which it has directly aimed—and if he can farther show that such modifications are inherited, he will certainly have made out the former part of his hypothesis, viz., that "intelligence, *i.e.*, action directed to a purpose, has been at work." This proof, however, he does not even attempt; but contents himself with asserting of 'adaptations' what is true, so far as we know, only of individual functional adaptations; and by thus using the word 'adaptations,' simply and without any expressed qualifications, he obtains a fictitious appearance of a demonstration; for the term 'adaptation,' when used in such arguments by evolutionists, is used in the sense of 'inherited structural adaptations'—as Professor Schiller ought to know perfectly well. It is simply the old metaphysical sophism of using one and the same word in two distinct senses, and crediting to the one sense of it what is readily granted to the other; and to perceive the fallacy involved in his use of the word 'adaptations' is to knock a pretty considerable hole in the bottom of Professor Schiller's argument. Can our author see no difference in kind between the functional adaptations of an individual man to a mari time life or a desert life and the structural adaptations of a whale and a camel to these respective modes of life? We all know that individual men, by varying their food and clothing, can thus intelligently adapt themselves to very different conditions of life; but how does this fact create any presumption that even the camel's stomach or the whale's form is the result of intelligent action directed to a purpose, and still less that the countless wonderful adaptations of which no man even is conscious until he studies anatomy, and over which even then he is powerless to exert any intelligent control, and which work best when he is unconscious —such adaptations, I mean, as the valves of the heart and countless others—are the outcome of intelligent action directed to a purpose? It does not indeed seem clear that even Lamarckian factors would avail Professor Schiller here, unless, possibly, the proverbial mouse could be shown to have acquired a neck a yard long by intelligently and purposefully directing its gaze to the unattainable cheese above it; for it were one thing to admit that the effects of use were inherited, but quite another to assert that any evolution has been due to the intelligent action of animals directed to the purpose of adapting themselves to their surroundings. Even were Mr Spencer's argument concerning the giraffe conceded by his opponents, I scarcely see

how this would help Professor Schiller; for not even the most fervid Lamarckian ever dreamed that the giraffe, when straining at the leaves, was intelligently and purposefully directing the development of muscles and bones and the rearrangement of his internal and external anatomy generally. Thus the very modifications, on which the whole value of his Lamarckian neck rested, were due to blind mechanical action over which he exercised no intelligent control. Moreover, it does not appear that Professor Schiller had even cleared up his notions of 'adaptations' so far as to think of appealing to Lamarckism to support his contention that intelligent action is responsible for many adaptations; for in that section he does not even mention Lamarck, though soon afterwards he remarks in passing (p. 872) that "it is practically certain that some Lamarckian influences must affect both the number and the character of the variations"—the metaphysician, with typical assurance and hastiness, thus dogmatically deciding a question over which our leading biologists, who alone are competent to speak authoritatively, are hopelessly at variance.

I pass over the curious passage in which Professor Schiller cites the action of the 'general physical and chemical laws of nature' as barring variations in certain directions, and thus rendering impossible the indefinite variation on which Darwin founded his arguments (p. 872),[1] and I will not comment here upon his strange citation of Bateson's work on Discontinuous Variation—which he fondly supposes to constitute a stumbling-block to Natural Selection—for I have already replied to that argument in the columns of *Natural Science* (May, 1895); but we will pass at once to the concluding section of Professor Schiller's article. This is really suggestive and ingenious; and, had the author excised the first ten pages of his article and retained only the latter part, he would probably have stood higher in the opinion of biologists. The pith of this latter part of his argument may be stated in very few words.

Darwin assumed that organisms vary indefinitely in every direction, and that the evolution of species is due to the action of natural selection in seizing upon and fixing a few among these countless variations. Were this assumption a literal statement of fact, any possibility of interpreting the universe teleologically would be barred *ab initio;* but, if variation be not indefinite in every direction, but more frequent in one direction than in others, it may be purposive; and thus the ground is cleared for building up a new teleology. Now Darwin's assumption was not a statement of fact, but a methodological assumption, exactly analogous to the economic assumption of an ideal 'economic man,'

[1] The same sort of objection might be brought against the first law of motion, and in either case is obviated by the insertion of the words 'tends to—'

or to the mathematician's assumption of a single body in an otherwise void space. By arguing from such methodological assumptions, the 'laws' of motion and the 'laws' of economics were obtained; but all these 'laws' are applicable to concrete facts, only with modifications, and after re-introducing the qualifications that were methodologically omitted from the premises, and the methodological assumption must never be accepted as a statement of literal facts.

Now this is certainly a very interesting and ingenious thesis, and I do not remember previously to have seen the suggestion made that Darwin's assumption was thus purely methodological; but it does not seem likely that Professor Schiller's ingenuity will be of any service to the teleologist. It were necessary before any teleological argument can be founded that he should prove (1) that variations are not indefinite but definite; and (2) that such definite variation can be attributed to no mundane factors, but can be explained only by the assumption of supra-mundane purposive intelligence.

The former hypothesis—for the sake of the argument—we will grant to Professor Schiller, although he has not even attempted to prove it, except by his curious remarks about 'chemical and physical laws,' &c.:[1] but what is it worth to him? Nothing! for that 'ultra-Darwinian' Weismann has already contended that variation is definite in direction, and he has offered a purely mechanical explanation of such definiteness;[2] so that what was to be treasured up as the trump card of the teleologist has already been played on the other side. But is it not indeed significant that the author of this curiously belated article, seeking to turn the evolutionists' flank and to clear the field for the teleologists, should be unaware that our most prominent living evolutionist had already, by anticipation, outflanked his flanking movement more than a year ago? Thus the only really at all valuable part of Professor Schiller's article, the one part not invalidated by fallacious trifling with words, is yet invalidated by his ignorance of the science that he seeks to press into the service of teleology. What, in this year 1897, can be more hopelessly belated than the following remarks (p. 875): "It is clear then that, to explain the changes which have resulted in the existing forms of life, some variable factor has to be added to natural selection. And as to the nature of that factor, Darwinism *qua* Darwinism tells us nothing." Perhaps it is even clearer that, had Professor Schiller possessed any acquaintance with

[1] It is one thing to argue that Darwin did not prove variation to be indefinite, quite another to prove that it is n o t intrinsically indefinite; and the teleologist must prove the latter proposition, and prove it by a wide induction from multitudinous details and experiments, before he can even talk of teleology.

[2] See his "Germinal Selection."

Weismann's work—of the whole of which, and, indeed, even of Weismann's existence, he seems to be profoundly ignorant—this passage, and the whole of his article besides, would have remained unwritten. It does seem to me rather hard that metaphysicians who want to write about evolution will not take the trouble to find out what evolutionism connotes at the present day; but Professor Schiller's ignorance of recent advances in biology is quite on a par with the curious and fatal misconception of the processes and factors of evolution that prevails throughout his article; and the cue to his philosophical status is afforded by his very theological conclusion, from which we learn that, if the whole of his argument be valid, evolutionism rather helps than hinders the teleologist, since he is no longer compelled to attribute perfection, but only gradual perfectioning, to this very imperfect scheme of nature, nor to saddle an omnipotent deity with the responsibility for deliberately planning and designing all the cruelty and suffering prevalent throughout the world. In other words, he proposes, by the help of evolution, to save divine morality at the expense of divine power; his directing Intelligence being, not an omnipotent fiend, but only an unpractised though well-meaning bungler. I hope that the theologians will be duly grateful to their very candid friend.[1]

[1] The theory of an innate tendency to vary in a definite direction of advance was, of course, advocated long ago by Nägeli. In an article on "Evolution and Teleology" that appeared in the *New Science Review* of July, 1895, I pointed out how strongly the recent advances in botany have told against this theory, and how in nearly every line of ascent evolution upwards has ended in a blind alley.

F. H. Perry Coste.

SOME NEW BOOKS

"Let us now Praise Famous Men!"

The Founders of Geology. By Sir Archibald Geikie. 8vo, pp. x + 298. London: Macmillan & Co., 1897. Price, 6s. net.

If, as Emerson has said, all history may be read in the lives of a few great men, this may be regarded as specially true of the history of science. The facts of science, no doubt, are accumulated by a multitude of workers; but ideas have their genesis in the brains of the leaders, and the growth of the ideas may best be studied as it took place originally in the minds of a few individuals. Hence Sir Archibald Geikie, when eager to dispel the lamentable ignorance of most of us concerning the historical development of geology, could have chosen no better means than this skilful and charming narration of the endeavours of the early pioneers.

The occasion of this work was the inauguration of the George Huntington Williams Lectureship at Johns Hopkins University, and one can imagine the delight with which American geologists listened to the polished periods and lucid exposition of Sir Archibald. Some of the perorations, indeed, are better adapted to the lecture-platform than the study, and an occasional weakness in the usually correct and forceful style suits the written less than the spoken word. Careful revision, for instance, would have eliminated such a sentence as this: "His father . . . died while the son was still very young, to whom he left a small landed property in Berwickshire." We test the author by his own high standard.

The geologists referred to in this book are—Leibnitz and Buffon (who, however, are not regarded as among the founders of geology, but as the last of the cosmogonists), Guettard, Desmarest, Pallas, De Saussure, Lehmann, Fuchsel, Werner, D'Aubuisson, Von Buch, Hutton, Sir J. Hall, Giraud-Soulavie, Cuvier, A. Brongniart, Omalius d'Halloy, Rev. J. Michell, William Smith, Murchison, Sedgwick, Logan, Agassiz, Nicol, Sorby, Lyell, and Darwin. Many others are mentioned incidentally and, in relation to these, the pioneers. For the expressed purpose of the book, this list is an excellent one. No doubt, every one that has read much in the early literature of geology will be ready with suggestions for its amendment. The fact is that a vast number of these old writers were not so ignorant or so foolish as we are too ready to suppose. Guettard is one whose claims have been strangely overlooked, and we are delighted to see this admirable appreciation of his many services to our science. But the list, it will be noticed, is almost entirely confined to French, German, and British geologists: Linnaeus and Wallerius are dismissed in a single line. Linnaeus, however, did more than arrange certain minerals in one of his kingdoms of nature: he studied the strata in which minerals and organised fossils occurred, travelling through Sweden and making

careful geological observations, which he published in the accounts of his journeys to Gotland, to Oeland, and, above all, to Westrogothia and Scania. He drew a section of the strata composing Kinnekulle, and paralleled them with beds in other parts of Sweden, subsequently using his knowledge to interpret the structure of Scania. "Thus," said he, "the section of Kinnekulle serves as introduction to *Strata terrae* or the anatomy of the earth-crust, not only here in Westrogothia, but probably over the greater part of the world." He recognised that the strata had been deposited in the sea throughout many long-vanished periods, and attempted to classify them according to their relative age. Thus Linnaeus laid the foundations of the Wernerian system before Werner was born; and it was not long before his fellow-countryman Bergman erected upon that foundation the actual framework that Werner filled in. Other Scandinavians might have been mentioned, such as Gyllenhahl, with his truly remarkable palaeontological study of *Echinosphaera*, and Hermelin, with his geological maps of Southern Norway and Sweden. The most curious omission, however, considering the occasion of the lectures, is that of citizens of the United States. Featherstonhaugh and H. D. Rogers are just alluded to, but that remarkable and much-abused geological genius, Ebenezer Emmons, is not even named.

We have not mentioned these omissions for the sake of fault-finding, but as further evidence of the amount of good work done by many whom it is the fashion of the present day to disregard. We sympathise warmly, as we have already said, with Professor Miall's recent plea for a more historical method of teaching the natural sciences, and, as a help in that direction, no book is better adapted than the present. But those who imagine, if such there be, that they have passed beyond the student stage, would yet do well to dip now and again into the battered volumes that grow dusty on topmost shelves. There are many observations and many shrewd suggestions hidden in those old books, made perhaps too early in the day to have taken effect, but waiting to be applied by us now with our modern knowledge and methods. Rosinus, for instance, 178 years ago, described the course of the nerve-canals, "*foramina jure meritoque pro nervorum canalibus reputanda*," in the cup of the lily-encrinite, in language that Dr W. B. Carpenter (the modern discoverer of that nerve-system) could not have bettered. The fact is that the worth of a man's work does not necessarily depend on the number of his years or on the century in which he lives.

Some of the most valuable passages in this book are those in which Sir Archibald uses the weight of his experience to enforce the morals to be derived from the study of the older writers. In one place he quotes Fitton's review of the Wernerian school: "A Wernerian geognost is chiefly employed in placing the phenomena he observes in the situations which his master has assigned to them in his plan of the mineral kingdom. It is not so much to describe the strata as they are, and to compare them with rocks of the same character in other countries, as to decide whether they belong to this or that series of depositions, supposed once to have taken place over the whole earth, . . . to ascertain their place in an ideal world." Similar criticism might justly be applied to-day in various branches

of science. Sir Henry Howorth's 'heretical' article in our November number did little more than translate the words of Fitton. Then are there not a good many hard-working zoologists and botanists who are perfectly satisfied if they can assign their specimens to certain pigeon-holes made for them in a rather hypothetical cupboard called a System? It is even a fact that many excellent old writers are ignored just because they could not, or would not, use a regular Linnaean terminology. Their works are neither read nor to be found in our scientific libraries. Into such obscurity even Hutton's great work, "The Theory of the Earth," may fall. It is nothing that we saw it characterised recently in the catalogue of a second-hand bookseller as "This extravagant theory which was defended by the celebrated Professor Playfair"; but we were indeed surprised to find no copy of it in the geological library at the Natural History Museum.

With reference to Playfair's defence, known as "Illustrations of the Huttonian Theory," Sir Archibald remarks: "For precision of statement and felicity of language, it has no superior in English scientific literature. To its early inspiration I owe a debt which I can never fully repay. Upon every young student of geology I would impress the advantage of reading and re-reading, and reading yet again, this consummate masterpiece. How different would geological literature be to-day if men had tried to think and write like Playfair!"

But it may be objected, How can we find time to read these old authors, much less to write like the best of them? We have to read the modern literature, and even a small part of that is overwhelming. We have so much to do that we cannot waste our energies on mere style, and we must rush out our results the easiest and quickest way we can, or we shall be anticipated. Sir Archibald's answer should be laid to heart, not merely by geologists old and young, but by all scientific workers. He finds his consolation in "the conviction, borne in upon us by ample and painful experience, that a very large mass of the geological writing of the present time is utterly worthless for any of the higher purposes of the science, and that it may quite safely and profitably, both as regards time and temper, be left unread. If geologists, and especially young geologists, could only be brought to realise that the addition of another paper to the swollen flood of our scientific literature involves a serious responsibility, that no man should publish what is not of real consequence, and that his statements when published should be as clear and condensed as he can make them, what a blessed change would come over the faces of their readers, and how greatly they would conduce to the real advance of the science which they wish to serve." There is not a dull page in "The Founders of Geology," but, were it only on account of this last paragraph, we should wish for it many readers in all parts of the world.

The Death of Rocks

A Treatise on Rocks, Rock-Weathering, and Soils. By G. P. Merrill. 8vo, pp. xx + 411, pls. xxv. New York: The Macmillan Company, 1897. Price, 17s.

What the unsatisfactory preservation of fossils is to a palaeontologist and surface drift to a stratigrapher, decomposition of rocks has long

been to the petrologist. Each is regarded as an unmitigated nuisance, interfering with the observation of facts for which the enquirer is searching; and as the stratigrapher is expected to map solid rock through its drift covering, the petrological specialist, when consulted as to the character of a rock, is expected to say, not what it is, but what it may once have been.

Professor Merrill, however, has endeavoured, in the work under review, to make the processes and results of weathering as interesting and full of real importance as the American and English glacialists are making the drift. For it is the weathering of rocks which forms soil, the link between the dead earth-crust and the living plants and animals upon it. Geologists have studied rocks in all their various kinds, their origin, differentiation, and metamorphism, their birth, growth, and life; we have now to study their death, and the earliest parts of the process which culminates in their resurrection.

In order to make his book self-contained, Professor Merrill devotes several chapters to considering the chemical and mineralogical composition structures, mode of occurrence, and various types of rocks; this is a fairly useful summary, but we are inclined to doubt whether the geologist will need, or the lay-reader understand it. Many of the illustrations in this part are admirable, and the abundant analyses are particularly valuable for reference later on. To many of these no references are appended, and we may conclude that they are due to the author himself, who must have expended a great deal of time and labour on the analyses throughout the work.

Part III. is devoted to the weathering of rocks, each of the chemical and mechanical agencies being taken in turn. Several valuable instances of the effect of alternations of high and low temperature are given, and the effect of cold rain on highly heated surfaces is referred to. While stress is laid on the effect of hydration, the work of carbonated rain-water, and of the humic, ulmic, and crenic acids naturally comes in for a lion's share of consideration. The action of the first of these solvents upon many silicates can be detected within ten minutes, while forty-eight hours' digestion will obtain from some amphiboles, epidotes, felspars, etc., quantities of lime, magnesia, iron, alumina and silica, amounting to from 0·4 to 1 per cent. of the mass. Hornblende is more easily acted upon than felspar, and even magnesian silicates are attacked, so that serpentine cannot be considered a final product of decomposition. Increasing the pressure on the solvent has much more effect than prolonging the time of its action. Daubrée's experiments on attrition are referred to, and the work of plants, bacteria, termites, and marine animals on the sea-bed, is not neglected.

Special cases of weathering are next treated in detail and illustrated by full analyses. Mere bulk analyses of the fresh and weathered rock are misleading, as they do not show all that has actually occurred. It is necessary to ascertain which constituents are least liable to be leached out, and to recalculate the analyses on the assumption that they remain constant. Alumina and iron oxides are least liable to this, and the analyses of acid rocks are worked out on this assumption. An example will show the value of this method; the one chosen illustrates, in addition, that while only 30 per cent. of

the original rock was soluble in hydrochloric acid and sodium carbonate, the proportion is now nearly 70 per cent. The instance is that of a gneiss from Virginia.

	FRESH GNEISS.		DECOMPOSED GNEISS.		CALCULATED AMOUNTS SAVED AND LOST.		
	Bulk analysis.	Soluble portion.	Bulk analysis.	Soluble portion.	Total loss.	Percentage of each constituent saved.	Percentage of each constituent lost.
SiO_2 .	60·69	10·09	45·31	17·69	31·90	47·55	52·45
Al_2O_3 .	16·89	13·54	26·55	24·86	0·00	100·00	0·00
Fe_2O_3 .	9·06		12·88	11·80	1·30	85·65	14·35
CaO .	4·44	1·64	Tr.	0·06	4·44	0·00	100·00
MgO .	1·06	0·89	0·40	0·37	0·80	25·30	74·70
K_2O .	4·25	2·40	1·10	0·75	3·55	16·48	83·52
Na_2O .	2·82	1·10	0·22	0·25	2·68	4·97	95·03
P_2O_5 .	0·25	0·23	0·47	—	0·00[1]	100·	0·00[1]
Loss on ignition	0·62	0·62	13·75	13·40	0·00[1]	100·	0·00[1]
	100·08	30·47	99·98	69·18	44·67		

[1] Ga'n.

This table makes at once obvious the great loss in silica, lime, and alkalies, and the gain in water; while it further indicates from the increase in the soluble portion that the state of combination in many of the compounds has changed. Mechanical separation and microscopic examination of the decomposed material throws further light on the character of the decomposition and nature of the surviving minerals.

In basic rocks the loss of silica is rather less, and that of iron and magnesia more conspicuous, while in a French basalt the felspars were the first to yield, and the augite and olivine the last.

Under the heading "Physical Manifestations" are treated: Disintegration without decomposition, influences of crystalline and rock-structure, mineral composition (roughening and crusting), induration and colour changes. Original characters are sometimes lost entirely through weathering, basalts pass into apparent argillaceous deposits, and granites and gneisses with their veins and every structural detail well preserved, may become so soft that a stick can be thrust deeply into them. Chemical compounds become on the whole simplified by weathering, but oxidation and all chemical change, except hydration, seems to cease below the permanent water-level. More refractory and dense residues, like xenotime, monazite, tourmalines, rutiles and precious gems, have tended to accumulate in favoured spots, on account of the weathering and destruction of the rocks in which they were originally contained.

The rate of weathering is influenced by composition, texture, and position of rocks, and by humidity of climate. Weathering in humid climates differs in kind as well as degree from that effected in dry climates as shown in California by the author and by Judd in the Nile delta. Prehistoric implements and old surfaces covered unconformably by newer deposits may sometimes enable us to guess at the amount of work done in a given time, but the ascertaining of the actual rate of work is difficult, and the results not very satisfactory. Geikie's results, on this branch of the subject, not quoted here by the author,

are important. A summary of the work of transporting agents concludes this portion of the work.

The fifth and last part of the book is devoted to the consideration of the regolith, a term used to include the soil which covers the solid rocks like a blanket (ῥηγος), an incoherent mass of varying thickness composed of materials essentially the same as those which make up the rocks themselves, but in greatly varying conditions of mechanical aggregation and chemical combination. The materials of the regolith are either sedentary or transported, the first class being either residual like wacke, laterite, terra rossa, &c., or cumulose like peat. Residual deposits are generally unstratified and may possess characteristics which they have 'inherited' from hundreds or even thousands of feet of rock which have totally disappeared. Transported deposits are colluvial (scree and avalanche debris), alluvial and aeolian, or glacial. Alluvia vary in character and to a striking extent in fertility according to whether the river bearing them drains a dry or a damp country; in the former case the undecomposed silicates will be rich in plant food. Where saline deposits are absent, indeed, the soils of very dry countries are of an extremely fertile character and only need rain to be covered with luxuriant vegetation, and this is especially the case because percolating rain has not been present to concentrate the clay into a 'hard-pan' below the surface.

The averages of several hundred analyses of soils in dry and humid regions show that the soluble constituents of the soil in arid tracts amount to 30 and in wet tracts to 15 per cent., the greatest differences being in the quantity of lime, magnesia, and alkalies.

Richthofen's aeolian hypothesis is considered satisfactory when applied to the Chinese loess, but not to that of America, which latter the author supposes to be the result of streams draining from the ice of the glacial period.

An important organic agent in the formation of marine deposits in inlets is found in the eel-grass, which grows vigorously as soon as a little mud has formed on the bottom, and then by deadening all currents promotes very rapid deposition of fine salt.

In dealing with the 'rock-flour' brought down by glaciers, Professor Merrill alludes to the small amount of actual clay present. It has been proved by Mr E. Dickson that what there is of this material is ground up felspar and not kaolin.

To illustrate the wonderful degree of comminution reached in soils, it is pointed out that the total surface area of the grains in a cubic feet of soil amounts on an average to 50,000 square feet. On areas like this the operation of organic acids, plants, animals, and water must be enormous. Ants working in such soil effect as much change as the earthworms studied by Darwin, and Shaler's calculation that the former bring a layer of soil one-fifth of an inch thick to the surface every year finds a close parallel in the figures obtained by Darwin.

Professor Merrill's book is an admirable introduction to a complicated and difficult subject. It is packed with facts, not perhaps arranged in the best possible manner, and illuminated with a wonderful series of analyses which contain a fund of valuable information. The author is modest in his deductions, not eager to generalise, and only adopting or advocating a conclusion after full discussion. It is to be

hoped that his work will lead to the more detailed study of the changes produced in rocks as they weather down and pass into their final, but by no means least useful, form. The illustrations are for the most part clear and well chosen, but it would be well to indicate the exact magnification of the diagrams of microscopic objects in the text.

The Volcanoes of North America

Volcanoes of North America: A Reading Lesson for Students of Geography and Geology. By Israel C. Russell, Professor of Geology, University of Michigan. 8vo. Pp. xiv+346, with 16 plates and 11 figs. in text. New York: The Macmillan Co.; London: Macmillan & Co., Ltd. 1897. Price, 16s. net.

The second title of this handsomely printed book must be regarded as that which expresses the intentions of its author. To the American reader, the work is an introduction to the study of volcanoes, comparable to those of Prof. Judd and others, which are already familiar to us in Europe. Only 170 pages, or half the book, deal with the volcanoes of North America; and it is to these that the professed geologist will most naturally turn. Considered, then, in its dual aspect, Prof. Russell's work must do much to stimulate curiosity and observation in the United States, by pointing out the absorbing interest of volcanic phenomena, and the extraordinary illustrations of the subject possessed by the North American continent.

Vesuvius, as seems inevitable, introduces the description of complex volcanic mountains; and the famous photographs of 1872 repeat themselves on the second plate. Nor will the beginner feel any irritation at perusing the graphic details provided by the younger Pliny. He is directed to Mr Lobley's book on Vesuvius for a history of the mountain; and we should have liked a reference also to the classic work of Phillips. Palmieri's experiences, and the report of the Krakatoa committee, are then drawn on; and these passages conclude with a remarkable quotation, from "one of our most profound students of volcanic phenomena" (p. 28), to the effect that the performances of Vesuvius "are mere Fourth of July fireworks in comparison with the Day of Judgment proceedings of Krakatoa." Prof. Russell's own good taste and style fortunately prevent his imitating this profundity, even where, as in Chapter VIII., he allows himself romantic possibilities.

Dutton and Dana furnish the account of Hawaii, and the Deccan trap and the Newark system of the Atlantic coast are quoted as examples of surface-flows of vast extent. In the latter instance, we may remind ourselves of the wide field of literature and 'solid geology' already traversed by Prof. Russell in the Eastern States.

The terms 'aa' and 'pahoehoe' seem to have gained vitality in geological literature (pp. 60-62), just as we might borrow with profit many of the names by which a Highlander or a Welshman designates the various forms of mountains. Prof. Russell refers the 'aa' type of lava-surface to imperfectly fluid streams, in opposition to what has been stated to occur upon the slopes of Vesuvius. The pahoehoe, on the contrary, flows easily and cools in thin sheets before it can break up, furnishing a smooth and often glassy surface.

The classification and description of igneous rocks professes only

to be a sketch, and granite is selected as the deep-seated type, followed by four lavas. Augite or soda-augite, as well as hornblende and biotite, should, we think, have been mentioned among the common constituents of rhyolite (p. 122) and trachyte (p. 124). We do not quite gather the meaning of the description of andesites "as generally dark, and mostly fine-grained rocks, with a restricted amount of glassy base, but larger than in the trachytes."

On p. 127 we commence the study of North American volcanoes, and it is interestingly pointed out (p. 133) that the two ends of the great western line are highly active, while there is a "middle region of extinct or, perhaps, in part, dormant volcanoes, extending from central Mexico through the western part of the United States and far into Canada."

Considering the enormous area to be covered, no one geologist can be expected to have an intimate acquaintance with North American volcanoes; and hence the description of those in Central America and Mexico is necessarily a compilation from works already published. So little is known of this district, however, that we are glad to be put in touch with the amazing history of Izalco, the growth of which, from nothingness to a height of 1500 feet, was witnessed by the curé of Sonsonate during his quiet country life, or the catastrophic explosion of Conseguina in 1835, which seems to have been connected with earthquakes that carried ruin into Chile. Among recent accounts —still far too few—we have those of Prof. Heilprin's ascents in Mexico. There seems some contradiction, however, between p. 178, where we are promised mountaineering excitements on Ixtaccihuatl, and the mere comparison with Popocatepetl on p. 183.

A country that possesses the denuded necks near Mount Taylor (Pl. 6, fig. B), and the exquisitely preserved Ice Spring craters of Utah (Pl. 7), is truly a paradise for the student; but the distances from point to point must still remain obstacles to research. The cone near Lassen's Peak, described by Diller, is of admirable freshness, and may have been active in the present century. Hence there are further volcanic possibilities in store for observers within the limits of the United States.

On p. 234 we gain a conception of the huge chain of peaks, still awaiting detailed enquiry, that runs north from San Francisco into Washington. These seem to result from Tertiary eruptions, and are not unworthy companions of the Andes, which are more familiar to us, owing to the pre-eminence of certain of their summits. The photograph of Mount Rainier (Pl. 14) may be cited, among the beautiful series of illustrations that adorns Prof. Russell's book.

The account of the Columbia lava (p. 250) will interest students of fissure-eruptions. Prof. Russell clearly points out that the enormous area, 200,000 to 250,000 square miles, is not buried in one vast flow; "the lava sheets overlap and supplement one another so as to form a continuous and highly compound system." Individual flows have, however, been traced on cañon-walls for a score or more of miles.

When we reach Alaska, we welcome the photographs of peaks and islands on Plates 15 and 16, and feel more than ever grateful for the records that Prof. Russell has brought together in the American

portion of his book. This portion concludes with an account of the great part played by volcanic dust in the soils of many of the western States. A reference to Prof. Judd's paper on the lavas of Krakatoa (*Geological Magazine*, 1888) would have excellently supplemented the important quotation from Diller on p. 293, to the effect that volcanic dust is richer in silica than its parent lava.

In the 'theoretical considerations' of chapter VII., we fancy that there is a certain amount of slaying of the slain. But this walk across the battlefield is in reality of service to learners, who are liable to regard all printed text-books as infallible. The suggestion on p. 314 that "volcanic activity increased with geological ages, and reached its maximum in Tertiary times," is confessedly based on the geological history of North America, and is, we fancy, not even well founded for that area. The amount of denudation that has laid bare the Archæan rocks of the north-east has probably wrought havoc along many old lines of volcanic activity.

Prof. Russell regards the water in lavas as collected during the passage of molten matter, moving under earth-pressure, from the lower into higher and waterlogged regions of the crust (p. 318); and the liquid matter may arise during local relief from pressure, as the product of rocks previously solid (p. 312). Hence he considers steam rather as a variable and unessential factor in determining a volcanic outburst.

The work is, as we have hinted, admirably produced by the publishers. The printers give us 'Roichthofer' for 'Richthofen' on p. 252, and, far more excusably, 'lavæ' and 'lava cases' for 'larvæ' and 'larva cases' on p. 209.

GRENVILLE A. J. COLE.

ANOTHER MEMOIR ON FUNAFUTI

THE ETHNOLOGY OF FUNAFUTI. By Charles Hedley. Australian Museum, Sydney. Memoir III., part 4, 1897.

THE pressing necessity of a systematic and immediate survey of the ethnology of the islands of the Pacific is again brought clearly before us by Mr Hedley's paper. It is only too manifest that the strictly native culture of Funafuti is rapidly dying out, that the older arts, customs, and appliances are changing apace under the influence of European missionaries and traders. In a few years' time the very recollection of the older culture will die out, and it will be impossible to obtain for our museums even models of the former appliances made with any accuracy at any rate. Hence every careful contribution to South Pacific ethnological literature must be welcomed. Mr Hedley's paper does not profess to be in any way an exhaustive monograph, but is, in the main, a descriptive list of the ethnological specimens and models collected by himself and others, and placed now in the Australian Museum at Sydney. Most of these are described clearly and in some detail, while their interest is increased by reference to the resemblances observable between the various native implements of Funafuti and those of other islands, as bearing upon their affinities and upon the probably complex origin of the general culture of the inhabitants of the island. Funafuti seems to have drawn its culture

from various directions. Though in the main Polynesian, more particularly Samoan, in general character, there is evidence in the arts and appliances of affinities with Micronesian culture, while even Melanesian traces are not wanting. The curious shell-bladed cocoanut scraper, mounted upon a wooden, elbow-shaped stock, belongs to a type of tool which has been recognised in Matty Island, to the north of New Guinea, while Mr Hedley might have added that the same is also found in the Solomon Islands, and that it crops up again in Ceylon, with a metal blade substituted for the shell. In North India, too, a nearly allied implement is found with a knife-blade replacing the scraper. The details given regarding the various types and the manufacture of fish-hooks are of importance, and point to affinities with the fish-hooks, both of the eastern and the western Pacific groups. The canoes are described in detail. The various implements, toys, etc., are too numerous even to be mentioned here, but all are recorded with care. It is a pity that the term 'drum' is applied to the hollow trough-shaped wooden instrument of Funafuti. This belongs to a very widely distributed type of instruments, which belongs essentially to the gong series, and should on no account be confused with the drums, which are characterised by a sounding medium of tense membrane. The vague descriptions of some travellers constantly confuse the two perfectly distinct instruments, and ethnologists should studiously avoid falling into the same error. Mr Hedley is wrong in supposing that the 'ploughing' method of producing fire by friction is the only one employed in the Pacific Islands. The simple twirling drill has been described from New Zealand, the New Hebrides, and Carolines, and other instances might be mentioned. In this, as in many other instances, a specialist would, no doubt, have added greatly to the information given in the paper, but at the same time Mr Hedley's contribution should prove a useful one, and welcome to ethnologists. HENRY BALFOUR.

A CATALOGUE OF MAMMALS

CATALOGUS MAMMALIUM, TAM VIVENTIUM QUAM FOSSILIUM, a Dr E. L. Trouessart. Nova editio (prima completa). Fasciculus II., Carnivora, Pinnipedia, Rodentia I. (Protrogomorpha and Sciuromorpha), pp. 219-452; Fasciculus III., Rodentia II. (Myomorpha, Hystricomorpha, Lagomorpha), pp. 453-664. Berlin: R. Friedländer & Sohn, 1897. Price, 10 marks each fasciculus.

THE second and third portions of this admirable and most useful list fully bear out the promise of the first part, noticed in *Natural Science* for May. They contain, besides the Carnivora and Pinnipedia, which the author separates ordinally, the whole of the rodents, the most difficult and most numerous order of mammals, and will therefore be most welcome to every working mammalogist. The list seems throughout to be remarkably complete and up to date, and we have scarcely been able to find a single omission. The print and get-up are even better than in the first parts, and the misprints due to some of the specific names being printed with capitals are reduced to a minimum (though not to *nil*). Acting on a suggestion in our previous notice the original localities for the names considered to be synonyms have been printed opposite the latter, so that it can be seen

at a glance what forms have been described from different parts of the main area.

En passant we may note two accidental mistakes—the first that a number of rodents from Mashonaland, described by Mr de Winton in 1896, have been wrongly credited to the present reviewer, and the second that by a confusion of two similar names, the marsupial *Thylamys carri*, of Trinidad, appears as a rodent in the genus *Tylomys*.

We may express a hope, in conclusion, that this invaluable list will not be allowed to come to an end without having a good index appended to it, at least of the genera names and their synonyms (with page, not number, references), as this will enormously increase the facility of its use, and will make it really worthy of its claim to be *Editio completa*. O. T.

Parasitic Flies

Revision of the Tachinidae of America North of Mexico. By D. W. Coquillett. Pp. 154. Washington. 1897. (U.S. Department of Agriculture, Division of Entomology. Technical Series, No. 7.)

Like most of the publications of the admirable Washington department, this work will be welcomed both by the systematic student and the practical farmer. The flies described are parasitic on various insects, usually on the caterpillars of moths, hence they play an important part in keeping the numbers of injurious species within bounds. The work contains tables of the parasites with their hosts and the hosts with their parasites, together with synoptical tables, and full descriptions of all the Nearctic genera of species. As most of the genera are identical with those of Europe, the monograph should be valuable to dipterologists generally. We notice with regret that there are no illustrations. In the study of so difficult a family, a few figures of structural details would be of great help to the beginner.

Further Literature Received

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On some European Slugs of genus *Arion*, and on two new species of *Parmarion*, W. E. Collinge: *Proc. Zool. Soc.* On Flow-Structure in an Igneous Dyke, and on Augite-diorites, by T. H. Holland: *Rec. Geol. Surv. India*. A Dictionary of the Call Notes of British Birds, C. L. Hett. Field Columbian Museum, Papuan Crania, G. A. Dorsey. Ratzel's History of Mankind, pts. 21, 22: Macmillan. U.S. Dept. Agriculture, N. Amer. Fauna, No. 13. Concilium Bibliographicum, cards relating to *Nat. Sci.*

Amer. Geol., Nov.; Amer. Journ. Sci., Nov.; Amer. Nat., Nov.; Botan. Gaz., Oct.; Feuille des Jeunes Nat., Nov.; Irish Nat., Nov.; Journ. School Geogr., Oct.; Literary Digest, Oct. 16, 23, 30, Nov. 6; Naturae Novit., Oct.; Naturalist, Nov.; Nature, Oct. 21, 28, Nov. 4, 11, 18; Naturen, Oct.; Photogram, Nov.; Psychol. Rev., Nov.; Rivista Quindicinale di Psichologia, vol. i., fasc. 9, 11 (Sept., Oct.); Revue Scientifique, Oct. 16, 23, 30, Nov. 6, 13; Science, Oct. 8, 15, 22, 29, Nov. 5; Scientific American, Oct. 9, 16, 23, 30, Nov. 6; Scot. Geogr. Mag., Nov.; Scot. Med. and Surg. Journ., Nov.; Victorian Nat., Sept., Oct.; Westminster Rev., Nov.

OBITUARIES

CHARLES SMART ROY

BORN AT ARBROATH, 1854. DIED AT CAMBRIDGE, OCTOBER 4, 1897

THE death of the professor of pathology in the University of Cambridge is a serious loss to the scientific study of that subject. Roy received his training at St Andrews and Edinburgh. His earliest research work, that on the pleuro-pneumonia of cattle, was conducted at London, chiefly in the Brown Institution, of which Burdon-Sanderson was head. Proceeding to Berlin, he studied in the laboratories of Virchow and Du Bois-Reymond, and produced his paper "On the influences which modify the work of the heart," published in the *Journal of Physiology*. In 1879 Roy became assistant at the Physiological Institute of Strassburg University under Prof. Goltz, where he invented the sphygmotonometer and other instruments for measuring the changes in blood-vessels. Here also he invented the well-known ether freezing microtome. Passing next to Cohnheim's Institute at Leipzig, he invented the renal oncometer for the study of variations in blood-flow through the kidney. From here in 1880 he came to Cambridge as George Henry Lewes' student in physiology, and worked in the laboratory of Prof. Michael Foster. In 1882 Roy succeeded Dr Greenfield as Director of the Brown Institution, a post which he held for two and a half years, during which time he visited the Argentine Republic to investigate a disease raging among the cattle of Entre Rios. In 1884 he was elected a Fellow of the Royal Society and to the newly-founded chair of pathology at Cambridge. His chief work was on the mammalian heart, partly carried out in conjunction with Prof. Adami. In prosecuting this research he invented yet other ingenious instruments. Among his students at Cambridge may be mentioned the names of Kanthack, Hankin, Griffiths, Lorrain-Smith, Cobbett, Lloyd-Jones, Rolleston, and Wesbrook. Many of these came as J. Lucas Walker's students in pathology, an endowment which he himself was largely instrumental in securing. Prof. Roy's health had been failing for some time, and for the past year his work at Cambridge has been undertaken by Dr Kanthack, who has now succeeded him.

PETER BELLINGER BRODIE

BORN 1815. DIED NOVEMBER 1, 1897

THE veteran geologist, the Rev. P. B. Brodie, who had been a Fellow of the Geological Society of London for more than sixty years, passed away on the first day of last month. He was born in London in 1815, and early proceeded to Cambridge, where he came under the influence

of Sedgwick, and was for some time volunteer assistant in the Woodwardian Museum. His first paper, on land and fresh water shells in association with mammalian bones in the gravels around Cambridge, was read before the Cambridge Philosophical Society so long ago as 1838, but he became best known by his numerous discoveries of fossil insects in the secondary rocks, and in 1845 he summarised all that was then known in reference to this subject in his small illustrated volume, "A History of the Fossil Insects in the Secondary Rocks of England," dedicated to his revered teacher Sedgwick. In the determination of the insects the author was assisted by the late Prof. J. O. Westwood. In 1838 Brodie entered the Church and became curate at Wylye, Wiltshire; in 1840 he removed to Steeple Claydon, Buckinghamshire; next year he was elected vicar of Down Hatherley, Gloucestershire; and in 1855 he became vicar of Rowington, near Warwick, where he died. In all these districts he accomplished much original work, and he contributed several papers to the Geological Society of London, besides others to the British Association and various Field Clubs. In recognition of the value of his labours the Geological Society awarded to him the Murchison Medal in 1887. He amassed a large collection of fossils, of which the more important specimens have been acquired by the British Museum, and he was indefatigable in his exertions to spread an interest in science among those by whom he was surrounded. In 1854 he was instrumental in founding the Warwickshire Naturalists' and Archaeologists' Field Club, and at the time of his death he was President both of this Club and of the Warwickshire Natural History and Archaeological Society. An excellent portrait of Mr Brodie appeared in the *Proc. Warwick Field Club* early this year, and it is reproduced, with an extended biographical notice, in the November number of the *Geological Magazine*.

ANDREW MATTHEWS

BORN JUNE 18, 1815. DIED SEPTEMBER 14, 1897

AT the ripe age of eighty-two one of the most accomplished of our clerical naturalists has passed away. The Rev. Andrew Matthews, who had held the living of Gumley, Leicestershire, since 1853, was a close observer of Nature, and interested in botany and ornithology. His fame will rest, however, on his studies of the family of minute beetles known as the Trichopterygidae. He published a beautifully illustrated monograph of these insects in 1872, and we are glad to know that the MSS. and drawings of a second volume had been completed by him in recent years, and will probably be issued shortly. He also contributed the account of these beetles to the "Biologia Centrali-Americana." All collectors of coleoptera are familiar with the catalogue of the British species of the order which he compiled in conjunction with Canon Fowler in 1883. That entomologist, in an appreciative notice of Mr Matthews in the *Entom. Monthly Mag.* for November, claims that the deceased naturalist was worthy to rank with Gilbert White as an observer, while he far surpassed the famous parson of Selborne as a close student of minute structure.

LOUIS CALORI

BORN FEBRUARY 8, 1807. DIED NOVEMBER 1897

LOUIS CALORI, the doyen of the Italian anatomists, was born at San Pietro in Casala, Bologna. His father was a doctor of medicine in practice at San Pietro, and young Louis attended the University of Bologna, becoming M.D. in 1829. In 1830 he was elected assistant professor of anatomy at his University; in 1835 he obtained the chair of comparative anatomy; and in 1844 that of human anatomy. He was ten times President of the Bologna Academy, and that body published a bibliographical list of his works in the fifth volume of the fourth series of the *Actes*. In the same year, 1884, the Academy gave a fête in his honour. Calori's chief zoological work was done among the Reptilia, but his researches threw light on many other groups of the animal kingdom.

CAPTAIN EDWARD YERBURY WATSON, who was killed by a sniper on November 8 at Simla while with Sir Wm. Lockhart's camp, was well known as an authority on the Hesperidae. He had arranged the collection at the British Museum before returning to India. Captain Watson was acting as Deputy-Assistant Commissary-General, and had seen a good deal of service in Burmah. He was promoted to the captaincy in 1895.

DR OTTO VOLGER, whose death was announced at the end of October, was a well-known German educationalist. His chief claim to posterity was his worship of Goethe and his care of the Goethehaus, but he was an indefatigable writer in the teaching of natural history and geology, both in Switzerland and Germany, and has left many minor works on those subjects.

The deaths are also announced of:—

HENRY CALDERWOOD, professor of moral philosophy in the University of Edinburgh, on Nov. 19; LEOPOLD AUERBACH and RUDOLF HEIDENHAIN, professors of physiology in the University of Breslau; HJALMAR HEIBERG, professor of pathological anatomy in the University of Christiania; E. LE GROS, professor of physiology in the new University of Brussels, aged 36; EDMUND DRECHSEL, professor of pharmacology in the University of Berne; GIUSEPPE FISSORE, sometime professor of pathology in the University of Turin, aged 82; W. MARME, director of the pharmacological institute of Göttingen; ALEXANDER MILTON ROSS, author of several works on the fauna and flora of Canada, at Montreal, Oct. 27; Dr MIETSCHKE, the German entomologist and naturalist; MAX SINTENIS, the German entomologist; ISAAC N. TRAVIS, taxidermist at the American Museum of Natural History; Rev. SAMUEL HAUGHTON of Trinity College, Dublin; Dr M. F. HEDDLE, late professor of mineralogy in the University of St Andrews; GEORGE HARRY PIPER, geologist of Ledbury, Herefordshire; JOHN CALVERT, mining expert, aged 86; and WILLIAM SCOTT, director of Royal gardens and forests, Mauritius, aged 38.

NEWS

THE following appointments are announced:—Dr A. A. Kanthack to be professor of pathology, and J. Graham Kerr to be demonstrator of animal morphology, in the University of Cambridge; Dr A. W. Sheen to be demonstrator of anatomy in the medical department of University College, Cardiff; Dr Max von Frey, of Leipzig, to be professor of physiology in the High School of Zurich; Dr W. Rothert, of Kazan, to be professor of botany and director of the physiological division of the botanical department of the University of Charkow; Henry S. Pritchett, of Washington University, St Louis, to be superintendent of the U.S. Coast and Geodetic Survey; William S. Carter to be professor of physiology in the University of Texas; Dr Arthur Allin to be professor of psychology in Colorado University; Dr Francis Kennedy to be demonstrator in experimental psychology in Princeton University.

SIR ARCHIBALD GEIKIE has been appointed Romanes Lecturer at Oxford for 1898.

DR HANS MOLISCH of Prague intends to spend the coming winter in botanical research at Buitenzorg, Java.

DR ARTHUR WILLEY has returned to Cambridge, where he proposes to investigate his collections during the winter.

THE tenth annual meeting of the Geological Society of America will be held in M'Gill University, Montreal, at the end of December.

LETTERS from Mr C. W. Andrews announce his safe arrival in Christmas Island, where he has already made some progress in collecting.

WE are pleased to learn from Sir Frederick M'Coy that our announcement of his retirement from his professorship in the University of Melbourne is premature.

'THE Louisiana Society of Naturalists' has been founded at New Orleans to investigate systematically the fauna and flora of the State of Louisiana, which has hitherto been much neglected.

THE new building for the Radcliffe Library at Oxford is to be towards the western end of the south front of the University Museum, that is to say, the end where the chemical laboratories are.

A LABORATORY for Experimental Psychology has, says the *Psychological Review*, been opened in the Illinois Eastern Hospital for the Insane, at Hospital, Ill., under the direction of Dr W. O. Krohn.

ACCORDING to *Science*, the University of the State of New York, following the plan it has adopted of lending to the schools libraries and pictures, offers to make loans of specimens of natural history from the State collections.

THE Annual Report of the Preston Scientific Society for 1896-97 records the accession of more than one hundred members during the year. Sections for technical scientific work have been formed, and we look forward to interesting results.

Dr Franz Steindachner superintends the zoological work on board the Austrian ship *Pola*, which this year continues the scientific exploration of the Red Sea, covering the ground between Jedda and Aden. Researches in physical oceanography will also be carried on.

Prof. Schauinsfeld, Director of the Bremen Museum, has returned with a large collection from a voyage of fourteen months in the Pacific. He spent some time on the small island of Laysan, and visited among other places the Sandwich Islands, Samoa, New Zealand, and Chatham Island.

The sum of £500 has recently been granted to the Manchester Museum at the Owens College for the purpose of installing the electric light. We understand that Mr Edward Holt's donation of £2500, lately announced, is to be devoted entirely to the rebuilding of the gymnasium of the College.

The Geological Circle of the Upper Holloway Centre of the London Society for the Extension of University Teaching will issue a strong programme for 1898. We expect great things from a Society with a name of such dimensions, and shall look for the programme with interest. Mr Rudler is the President.

The Imperial Russian Geographical Society is sending an expedition to Abyssinia, chiefly for the anthropological investigation of the country. The leader of the party is Mr Nicolas Dmitrieff, of Astrakhan, who has had some experience of the country as medical volunteer with the Italian army.

Messrs Schuchert and White, of the U.S. National Museum, have returned from Lieut. Peary's las Greenland expedition with a collection of Cretaceous and Miocene plants, as well as many Cretaceous invertebrates from the Noursoak peninsula. They seem to think the last word on these plants was said by Heer, which is hardly the view of European palaeobotanists.

The Jersey Natural Science Association is wisely including in its programme economic questions of general interest. At the October meeting Mr J. Hornell read an important paper on "The Possibilities of Fishery Improvement in Jersey, with Notes on the present state of Marine Pisciculture and Fishery Regulation." It appears in full in the *Jersey Weekly Press* of October 16.

One of the results of the terrible famine in India last year shows itself in the increase of deaths from wild animals. The greatest increase was in the North-West Provinces from wolves, and in the Sunderbunds from tigers, and was, no doubt, the result of a more vigorous search for food by the natives in the jungles. Snake-bites, however, decreased considerably, there being only 21,000 deaths in the year.

An Act was passed at the last meeting of the West Virginia Legislature, establishing a State Geological and Economic Survey in connection with the W. Virginia University, Morgantown. For its expenses a sum of 3000 dollars *per annum* has been appropriated. Dr Israel C. White is Superintendent, Prof. S. B. Brown is First Assistant Geologist and Curator of the Collections, and Professor J. L. Johnson is Assistant Geologist.

The Hull Scientific and Field Naturalists' Club opened its winter session by an Exhibition and Conversazione which ran for two nights—namely, November 10th and 11th, from 7.30 to 10 P.M. The whole of the exhibits were from the collections of members of the club, and testified to commendable activity in many right directions. The President of the club is Dr J. Hollingworth, and the Secretary is T. Sheppard, 78 Sherburn Street, Hull. The annual subscription is four shillings.

Science announces that the United States Geological Survey has practically completed the distribution of the Educational Series of Rocks, 175 suites of 156 specimens each having been sent out during the past summer to universities, colleges, and technical institutions in the United States. There remains a small number of incomplete sets, which will be placed in such smaller colleges as will make them most useful. The Educational Series were prepared by the Survey with much care, for the purpose of aiding students in acquiring a general and special knowledge of rocks and promoting the study of geology.

THE illustrations of geological sections as aids to the geologist who ventures into a museum are familiar to everyone who visits the museums at Jermyn Street, Cromwell Road, and many places on the Continent and in America ; but there is one particular section—that built in the gardens of the Landwirthschaftliche Institut of the University of Halle, in honour of Dr Julius Kuehn—which we do not think has been brought to the notice of readers of this journal. It is built up of the rocks themselves, and represents a section through the mountainous district of north and middle Germany. This very striking representation of geology was described by Professor K. v. Isitsch as long ago as 1891. Besides forming an unique memorial to Dr Kuehn, it has considerable value for the teaching of geology.

WE learn from the *Shooting Times* that the Guildford Natural History Society have been considering the question of the preservation of Wolmer Forest, which is only fifteen miles from that town, and have decided to present a petition to the Commissioners of Woods and Forests, praying that Wolmer Forest may be reserved as a sanctuary for wild birds, in which they, their nests, and eggs may remain unmolested throughout the year ; that it may not be let at any time for game preserving, or for any purpose inimical to bird life ; and that it may remain in perpetuity as a national memorial to the greatest outdoor naturalist England has produced—Gilbert White of Selborne. Such a recognition, the society urge, would show that the admiration of Gilbert White in the nineteenth century was so practical as to be of value to the naturalist and the English-speaking race for all succeeding time. The society have no wish to attempt to interfere with the use of the forest by the War Office for the purposes of military manoeuvres.

AN editorial comment in the *American Naturalist* for October includes some complimentary remarks on the British Association, which will be read in this country with interest :—"We may be pardoned if we point out some features in which we think the British Association superior to our own. In the first place, the Presidential Addresses delivered before the British Association strike us as, on the whole, better than those with which our audiences are greeted. While now and then an American address will rise to as high a standard as anything that Great Britain can boast, theirs are on the average the more thoughtful and scholarly, while ours too often have a perfunctory air and lack in breadth of view. In personnel of those who attend, the British Association again has the advantage. In England it is the fashion to attend these annual meetings, and no one there has reached such a pinnacle of greatness that he can afford to ignore or neglect this national society. As a result, at their gatherings one can be reasonably certain of meeting most of those who are the leaders in English scientific thought. In America, on the other hand, the tendency is in the other direction. It would be an easy matter to give a considerable list of names of those prominent in American science whose faces are never seen at the association meetings."

CORRESPONDENCE

LACÉPÈDE'S TABLEAUX. . . . DES MAMMIFÈRES ET DES OISEAUX ; 1799

LACÉPÈDE'S "Mémoire sur une nouvelle table méthodique des animaux à mamelles" was read before the Institute on 21 Prairial an 7 [9 June 1799]. It was published in the Mémoires de l'Institut, vol. iii., in 1801. Louis Agassiz quotes the table as Mém. de l'Instit., iii., 1797, an obviously incorrect date ; but many others have quoted it as 1799 without giving any evidence as to the accuracy of the quotation. As great importance attaches to the proper date of this paper, it became necessary for me to investigate the matter carefully, but I could not discover any definite statement except that of Engelmann, who in his Bibl. Hist. Nat. 1846, p. 376, refers to it as "in-4. *Paris. an* vii. (1799). Plassan. (38 pag.)." Up to the present, however, I have completely failed to find this 4to tract ; it cannot be a separate of the Mém. de l'Inst. paper because that was printed by Baudouin, and Engelmann gives Plassan as the printer of the tract ; besides the pages of the Institute paper are 32 against 38 of the tract referred to by Engelmann. But I have found, quite by accident, in Didot's issue of Buffon's Hist. Nat., 18mo *Paris*, 76 vols., 1799-1806, in the xiv. vol. of Quadrupedes, this interesting tableau, and it is dated 1799, and it was printed by Plassan. The title of the two papers (for the birds are included) are as follows :—"Tableau des divisions, sous-divisions, ordres et genres des Mammifères, Par le C^en^ Lacepède ; Avec l'indication de toutes les espèces décrites par Buffon, et leur distribution dans chacun des genres, par F. M. Daudin" (pp. 143-196).

"Tableau des sous-classes, divisions, sous-divisions, ordres et genres des Oiseaux, Par le C^en^ Lacepède ; Avec l'indication de toutes les espèces décrites par Buffon, et leur distribution dans chacun des genres, par F. M. Daudin" (pp. 197-346). Both of these are referred to as one tract, "in-18. *Paris*, 1802. Plassan," by Engelmann, Bibl. Hist. Nat. 1846, p. 322.

It will be noted first—That the *Genera only* are by Lacépède ; and secondly—that *all* the species are by F. M. Daudin.

As the period 1798-1801 is a critical one for nomenclature, I need do no more than point out the interest of this recovery ; but my friend Oldfield Thomas hopes to prepare an analysis of the Mammalia. Ornithologists will find many interesting points to consider in nomenclature when comparing the above paper with Daudin's "Traité Elémentaire," which it undoubtedly precedes.

C. DAVIES SHERBORN,
17*th Nov.* 1897. INDEX ANIMALIUM.

MR R. QUICK writes from the Horniman Museum, that an examination of Mr Harrison's collection of flints from the Plateau gravels of Kent has convinced him that these are truly primitive implements. He thus agrees with Sir Joseph Pretwich, and considers that Mr Cunnington's reasoning in our last number (pp. 327-333) is not cogent.

ERRATUM.—Page 321, line 25. *For* 'Siberian' *read* 'Silurian.'

NOTICE

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